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THE TRANSACTIONS

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THE TRANSACTIONS

OF THE

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

VOL. VIII.—NEW SERIES.

SESSION 1888-89.

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PREFACE.

THE present Volume is the *Eighth* of the *New Series*, and contains a record of the work done during the past Session.

That work, as hitherto, embraces the communication of Original Papers; the exhibition of Patients, illustrating rare and interesting forms of disease; and the exhibition of Pathological and other specimens, so essential to the proper understanding of the morbid changes which take place in the human body.

During the past Session three Extra Meetings were held for Discussions on Special Subjects,—one on the Necessity for further Legislation for the Care of Habitual Drunkards, introduced by Professor Grainger Stewart; another on the Pathology and Treatment of Strumous Glands, opened by Professor Annandale and Dr John Duncan; and a third on Intestinal Obstruction, its Pathology and Treatment, Medical and Surgical, introduced by Mr A. G. Miller. It is hoped that such Meetings will materially increase the usefulness of the Society.

It is believed that the publication of the Transactions in this permanent form will prove a valuable contribution to medical literature, will encourage the Members to take a more active part in the work of the Society, and will tend in no small degree to increase the influence and usefulness of the Medico-Chirurgical Society of Edinburgh.

WILLIAM CRAIG,
Editor.

September 1889.

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130	F. W. Dyce Fraser, M.D., F.R.C.P. Ed., Gorton House, Hawthornden,	1883
	Robert Henry Blaikie, M.D., F.R.C.S. Ed., 9 Palmerston Road,	1883
	R. M'Kenzie Johnston, M.D., F.R.C.S. Ed., 44 Charlotte Square,	1883
	* Charles Walker Cathcart, M.B., F.R.C.S. Eng. and Ed., 8 Randolph Crescent,	1883
	Alexander Bruce, M.D., F.R.C.P. Ed., 13 Alva Street,	1883
135	Andrew Semple, M.D., F.R.C.S. Ed., Dep. Surgeon-General, 10 Forres Street,	1883
	William Hy. Shirreff, M.B., C.M., Melbourne, Australia,	1883
	John Lyon Wilson, L.R.C.P. Ed., 4 Buccleuch Place,	1883
	Henry Newcombe, M.D., F.R.C.S. Ed., 5 Dalrymple Crescent,	1883
	* Francis Troup, M.D., M.R.C.P. Ed., 1 Minto Street, *Treasurer*,	1883
140	Russell Eliott Wood, M.B., F.R.C.S. Ed., 9 Darnaway Street,	1883
	John Macdonald Brown, M.B., F.R.C.S. Eng. & Ed., 12 South Mansionhouse Road,	1883
	James William Beeman Hodsdon, M.D., F.R.C.S. Ed., 30 Walker Street,	1883
	John Haddon, M.D., C.M., Marsh House, Canonbie	1883
	Germon Sims Woodhead, M.D., F.R.C.P. Ed., 6 Marchhall Crescent,	1883

		Date of Admission.
145	Thomas Francis Spittal Caverhill, M.B., F.R.C.P. Ed., 8A Abercromby Place,	1883
	Robert Alexander Lundie, M.B., B.Sc., F.R.C.S. Ed. 35 Warrender Park Road,	1883
	Professor Arthur W. Hare, M.B., F.R.C.S. Ed., M.R.C.S. Eng., 23 St John Street, Manchester,	1883
	Edwin Baily, M.B., C.M., Oban,	1883
	Alexander Black, M.B., F.R.C.P. Ed., 13 Howe Street,	1883
150	Harry Melville Dunlop, M.D., F.R.C.P. Ed., 20 Abercromby Place,	1883
	George Andreas Berry, M.B., F.R.C.S. Ed., 31 Drumsheugh Gardens,	1883
	Hamilton Wylie, M.B., C.M., 1 George Place,	1883
	Arthur Douglas Webster, M.D., M.R.C.P. Ed., 20 Newington Road,	1883
	Robert William Philip, M.D., F.R.C.P. Ed., 4 Melville Crescent,	1883
155	Joseph Carne Ross, M.D., F.R.C.P. Ed., 98 Mosley Street, Manchester,	1884
	William Russell, M.D., F.R.C.P. Ed., 46 Albany Street,	1884
	George Dickson, M.D., F.R.C.S. Ed., 9 India Street,	1884
	Thomas Wyld Pairman, L.R.C.P. & S. Ed., Te Awamutu, Waipa, Auckland, N.Z.,	1884
	Alexander Thom, M.D., C.M., Crieff,	1884
160	Hugh Logan Calder, M.D., C.M., 60 Leith Walk,	1884
	James Craig Balfour, L.R.C.P. & S. Ed., Seabiton House, Malvern Wells, Worcestershire,	1884
	Frederick Anastasius Saunders, L.R.C.P. & S. Ed., Denburn, Crail,	1884
	Wm. Richardson, M.D., F.R.C.S. Ed., Bath Lodge, Reading,	1884
	Andrew Brown, M.D., M.R.C.P. Ed., 1 Bartholomew Road, Kentish Town, London, N.W.	1884
165	G. J. H. Bell, M.B., C.M., Surgeon, Bengal Army,	1884
	T. Goodall Nasmyth, M.D., D.Sc., Cowdenbeath, Fife,	1884
	Henry Hay, M.B., C.M., 7 Brandon Street,	1884
	Thomas R. Scott, M.D., C.M., Musselburgh,	1884
	R. Milne Murray, M.B., F.R.C.P. Ed., 10 Hope Street,	1884
170	A. S. Cumming, M.D., F.R.C.P. Ed., 18 Ainslie Place,	1884
	Ernest F. Neve, M.D., F.R.C.S. Ed., M.R.C.S. Eng., Hospital, Srinagar, Kashmir, N.W. India,	1884
	W. C. Greig, M.B., C.M., Australia,	1884
	William Wilson, M.B., C.M., 2 N. Charlotte Street,	1885
	John Mowat, M.D., 1 Hope Park Terrace,	1885
175	Skene Keith, M.B., F.R.C.S. Ed., 42 Charles Street, Berkeley Square, London, W.,	1885
	D. Noël Paton, M.D., F.R.C.P. Ed., 4 Walker Street,	1885
	George Hugh Mackay, M.B., C.M., Elgin,	1885
	J. Michael Dewar, M.B., C.M., 24 Lauriston Place,	1885
	Edward M'Callum, F.R.C.S. Ed., 3 Brandon Street,	1885
180	T. Edgar Underhill, M.D., F.R.C.S. Ed., Bromsgrove, Worcestershire,	1885
	John Struthers Stewart, L.R.C.P. & S. Ed., 16 Merchiston Terrace,	1885
	Allen Thomson Sloan, M.D., C.M., 22 Forth Street,	1885
	John William Ballantyne, M.D., F.R.C.P. Ed., 50 Queen St.,	1885
	James Robertson Crease, F.R.C.S. Ed., 2 Ogle Terrace, South Shields,	1885
185	George Kerr, M.B., C.M., 6 St Colme Street,	1885
	Tom Bairstow, L.R.C.P. & S. Ed., 14 Buccleuch Place,	1885
	David Milligan, M.B., C.M., 11 Palmerston Place,	1885
	George Dods, M.D., L.R.C.S. Ed., 50 Great King Street,	1885
	J. Murdoch Brown, M.B., F.R.C.P. Ed., 9 Walker Street,	1885

		Date of Admission.
190	Robert W. Felkin, M.D., F.R.C.S. Ed., 20 Alva Street,	1885
	S. Hale Puckle, M.B., C.M., Bishop's Castle, Shropshire,	1885
	James Haig Ferguson, M.B., M.R.C.P. Ed., M.R.C.S. Eng., 25 Rutland Street,	1885
	Charles Kennedy, M.D., C.M., 25 Newington Road,	1886
	William Gayton, M.D., M.R.C.S. Eng., Bartram Lodge, Fleet Road, Hampstead, London, N.W.,	1886
195	Reginald Ernest Horsley, M.B., C.M., 46 Heriot Row,	1886
	James Mill, M.B., C.M., 178 Ferry Road,	1886
	Robert Fraser Calder Leith, M.B., B.Sc., 129 Warrender Park Road,	1886
	Thomas M. Burn-Murdoch, M.B., C.M., 31 Morningside Road,	1886
	Professor William Smith Greenfield, M.D., F.R.C.P. Lond. and Ed., 7 Heriot Row,	1886
200	Oswald Gillespie Wood, M.D., F.R.C.S. Ed., Surgeon, Army Medical Staff. India,	1886
	James Hogarth Pringle, M.B., C.M., 5 Livingstone Place,	1886
	Nathaniel Thomas Brewis, M.B., F.R.C.P. Ed., 59 Queen Street,	1886
	John Batty Tuke, jr., M.B., F.R.C.P. Ed., Balgreen, Murrayfield,	1886
	David Berry Hart, M.D., F.R.C.P. Ed., 29 Charlotte Square,	1886
205	Walter Scott Lang, M.D., F.R.C.S. Ed., M.R.C.S. Eng., 1 Leopold Place,	1886
	Alfred Bell Whitton, M.B., C.M., Aberchirder,	1886
	Robert S. Aitchison, M.B., F.R.C.P. Ed., 74 Great King Street,	1887
	J. A. Armitage, M.B., C.M., 15 Waterloo Road, Wolverhampton,	1887
	J. Walton Hamp, L.F.P.S. Glasg., L.S.A. Lond., Wolverhampton,	1887
210	William Hunter, M.D., M.R.C.S. Eng., 16 Panton Street, Cambridge,	1887
	Sydney Rumboll, L.R.C.P. and S. Ed., Belgaum House, Leeds,	1887
	John Thomson, M.B., F.R.C.P. Ed., 14 Coates Crescent,	1887
	George Franklin Shiels, M.D., C.M., 229 Geary Street, San Francisco,	1887
	T. Brown Darling, M.B., C.M., 36 South Bruntsfield Place,	1887
215	John Keay, M.B., C.M., Mavisbank House, Polton,	1887
	John F. Sturrock, M.B., C.M., 9 James' Place, Broughty Ferry,	1887
	Edward Carmichael, M.D., 8 Mansfield Place,	1887
	Charles C. Teacher, M.B., C.M., 16 Newington Road,	1887
	David W. Aitken, M.B., C.M., 3 Argyle Place,	1887
220	Robert Inch, M.B., C.M., Gorebridge,	1887
	John Shaw M'Laren, M.B., F.R.C.S. Ed., 14 Walker Street,	1887
	George Mackay, M.D., F.R.C.S. Ed., M.R.C.S. Eng., 2 Randolph Place,	1887
	Henry Alexis Thomson, M.D., F.R.C.S. Ed., 6A Bruntsfield Place,	1887
	David Wallace, M.B., C.M., 66 Northumberland Street,	1887
225	John C. Messer, M.D., R.N., 1 Lansdowne Crescent,	1887
	Thomas W. Dewar, M.B., F.R.C.P. Ed., 4 Stafford Street,	1887
	D. H. Anderson, M.B., C.M., Borough Asylum, Hull,	1887
	James Lockhart Wilson, M.B., C.M., Duns,	1888
	William Booth, F.R.C.S. Ed., 2 Minto Street,	1888
230	John M'Fadyean, M.B., C.M., 9 East Hermitage Place, Leith,	1888
	Thomas Russell, L.F.P.S. Glasg., Davidson's Mains,	1888
	T. Home Ross, M.B., M.R.C.P. Ed., 40 York Place,	1888
	George M. Johnston, M.D., C.M., 9 Morton Street, Leith,	1888
	George Pirrie Boddie, M.B., C.M., 28 South Bruntsfield Place,	1888
235	Kenneth Mackinnon Douglas, M.D., C.M., 30 Melville Street,	1888
	George Lovell Gulland, M.B., M.R.C.P. Ed., 6 Randolph Place,	1888
	William Burns Macdonald, M.B., C.M., Port Lodge, Dunbar,	1888

		Date of Admission.
	James Williamson Martin, M.D., M.R.C.P. Ed., Gie Appantoo, Axim, Gold Coast, Africa,	1888
	Charles H. Bedford, M.B., M.R.C.S. Eng., 2 Windsor Street,	1889
240	William Haldane, M.D., F.F.P.S. Glasg., Viewforth, Bridge of Allan,	1889
	John Hugh Alex. Laing, M.B., C.M., 11 Melville Street,	1889
	Harold Jalland Stiles, M.B., C.M., 5 Castle Terrace,	1889
	John Smith, M.D., M.R.C.S. Eng., 16 Townsend Crescent, Kirkcaldy,	1889
	Allan Cuthbertson Sym, M.D., C.M., 144 Morningside Road,	1889
245	Edmund Frederick Tanney Price, M.B., C.M., 28 Mayfield Road,	1889
	John Berry Haycraft, M.D., Sc.D., 20 Ann Street,	1889
	Henry Harvey Littlejohn, M.B., C.M., 24 Royal Circus,	1889
	Albert Edward Morison, M.B., M.R.C.S. Eng., Brougham Terrace, Hartlepool,	1889
	William George Sym, M.D., C.M., 58 Queen Street,	1889
250	Benjamin D. C. Bell, L.R.C.P. & S. Ed., Kirkwall,	1889
	Hugh Jamieson, M.B., C.M., care of A. W. Hutton, Esq., M.A., 479 Avenida Montesdeoca, Buenos Ayres,	1889
	David Gair Braidwood, M.B., C.M., Durhambank House, Bonnyrigg,	1889

NON-RESIDENT.

	Arthur Edward Turnour, M.D., M.R.C.S. Eng., *Denbigh*,	1843
	W. Ord M'Kenzie, M.D., L.R.C.S. Ed., *London*,	1845
255	W. Judson Van Someren, M.D., L.R.C.S. Ed., *Redhill, Surrey*,	1845
	William H. Lowe, M.D., F.R.C.P. Ed., *Wimbledon*,	1845
	George Skene Keith, M.D., F.R.C.P. Ed., *Currie*,	1845
	Veitch Sinclair, L.R.C.P. and S. Ed., *London*,	1850
	Andrew Graham, M.D., Fleet Surgeon, R.N.,	1853
260	Archibald Hall, M.D., *Montreal*,	1853
	W. Overend Priestley, M.D., LL.D., F.R.C.P. Ed., *London*,	1854
	Horatio Robinson Storer, M.D., *Newport, Rhode Island, U.S.*,	1855
	James C. Howden, M.D., *Montrose*,	1856
	Thomas Skinner, M.D., L.R.C.S. Ed., *London*,	1856
265	Professor William Smoult Playfair, M.D., LL.D., F.R.C.P.L., *London*,	1857
	J. Ivor Murray, M.D., F.R.C.S. Ed., *Scarboro'*,	1857
	Andrew Scott Myrtle, M.D., L.R.C.S. Ed., *Harrogate*,	1859
	Robert Foulis, M.D., F.R.C.S. Ed., *Cupar-Fife*,	1859
	Francis Robertson Macdonald, M.D., *Inveraray*,	1860
270	Professor John Young, M.D., *University of Glasgow*,	1860
	Norman Bethune, M.D., F.R.C.S. Ed., *Toronto*,	1861
	George Thin, M.D., L.R.C.S. Ed., *London*,	1861
	Peter Gordon, L.R.C.P. and S. Ed., *Juniper Green*,	1861
	J. Cecil Phillippo, M.D., *Kingston, Jamaica*,	1861
275	Professor William Stephenson, M.D., F.R.C.S. Ed., *Aberdeen*,	1861
	J. S. Beveridge, M.R.C.P. Lond., F.R.C.S. Ed., *Edenbridge*,	1861
	David Yellowlees, M.D., LL.D., F.F.P.S. Glasg., *Glasgow*,	1862
	Prof. Arthur Gamgee, M.D., F.R.C.P. Ed., F.R.S., *London*,	1864
	Professor John Cleland, M.D., LL.D., *The University, Glasgow*,	1864
280	R. B. Finlay, M.D., M.P., *Middle Temple, London*,	1864
	Stanley Lewis Haynes, M.D., M.R.C.S. Eng., *Malvern*,	1864
	Francis D. A. Skae, M.D., *Lerwick*,	1864
	James Watt Black, M.D., F.R.C.P.L., *London*,	1865
	David Brodie, M.D., *Canterbury*,	1865
285	Thomas Sheriff, L.R.C.P. and S. Ed., *Edinburgh*,	1867
	Peter Maury Deas, M.B., L.R.C.S. Ed., *Exeter*,	1868
	Professor J. G. M'Kendrick, M.D., F.R.C.P. Ed., *University, Glasgow*,	1870

		Date of Admission.
	Professor Lawson Tait, M.D., F.R.C.S. Ed. and Eng., LL.D., *Birmingham*,	1870
	J. G. Sinclair Coghill, M.D., F.R.C.P. Ed., *Ventnor*,	1870
290	James Johnston, M.D., F.R.C.S. Ed., *London*,	1871
	J. William Eastwood, M.D., M.R.C.P.L., *Darlington*,	1871
	Professor J. Bell Pettigrew, M.D., LL.D., F.R.C.P. Ed., *University of St Andrews*,	1873
	John Aymers Macdougall, M.D., F.R.C.S. Ed., *Carlisle*,	1875
	Thomas John Maclagan, M.D., M.R.C.P.L., *London*,	1875
295	Dr Groesbeck, *Cincinnati*,	1875
	Professor David James Hamilton, M.B., F.R.C.S. Ed., *Aberdeen University*,	1876
	J. Moolman, M.B., C.M., *Cape of Good Hope*,	1877
	Robert Somerville, M.D., F.R.C.S. Ed., *Galashiels*,	1877
	Graham Steell, M.D., M.R.C.P.L., *Manchester*,	1877
300	Frederick William Barry, M.D., D.Sc., *London*,	1878
	John Brown, M.D., F.R.C.S. Eng., *Burnley*,	1878
	Walter Weir, M.B., F.R.C.P. Ed., *London*,	1879
	Keith Norman Macdonald, M.D., F.R.C.P. Ed., *Cupar-Fife*,	1880
	John Home Hay, M.D., M.R.C.S. Eng., *Alloa*,	1880
305	John Mackay, M.D., L.R.C.S. Ed., *Aberfeldy*,	1881

ORDINARY MEMBERS

ARRANGED ALPHABETICALLY.

RESIDENT.

	Dr J. O. Affleck, 38 Heriot Row,	1871
	Dr R. S. Aitchison, 74 Great King Street,	1887
	Dr D. Aitken, 3 Argyle Place,	1887
	Dr D. H. Anderson, Borough Asylum, Hull,	1887
5	Dr James Andrew, 2 Atholl Crescent,	1869
	Professor Annandale, 34 Charlotte Square,	1863
	Dr Archibald, Woodhouse-Eaves, Loughborough,	1882
	Dr J. A. Armitage, 15 Waterloo Road, Wolverhampton,	1887
	Dr W. Badger, Penicuik,	1882
10	Dr J. Johnson Bailey, Marple, Cheshire,	1874
	Dr Edwin Baily, Oban,	1883
	Tom Bairstow, Esq., 14 Buccleuch Place,	1885
	Dr Andrew Balfour, Portobello,	1874
	Dr J. H. Balfour, Portobello,	1881
15	Dr G. W. Balfour, 17 Walker Street,	1874
	Dr James Craig Balfour, Seabiton House, Malvern Wells, Worcestershire,	1884
	Dr Thomas Balfour, 51 George Square,	1856
	Dr Alexander Ballantyne, Dalkeith,	1872
	Dr J. W. Ballantyne, 50 Queen Street,	1885
20	Dr A. H. Freeland Barbour, 24 Melville Street,	1881
	Dr C. H. Bedford, 2 Windsor Street,	1889
	Dr Benjamin D. C. Bell, Kirkwall,	1889
	Dr G. J. H. Bell, Surgeon, Bengal Army,	1884
	Joseph Bell, Esq., 2 Melville Crescent,	1862
25	G. H. Bentley, Esq., Kirkliston,	1877
	Dr G. A. Berry, 31 Drumsheugh Gardens,	1883
	Dr Alexander Black, 13 Howe Street,	1883
	Dr W. T. Black, 2 George Square	1877
	Dr Robert H. Blaikie, 9 Palmerston Road,	1883
30	Dr Bleloch, 2 Lonsdale Terrace,	1871

		Date of Admission.
	Dr G. P. Boddie, 28 South Bruntsfield Place,	1888
	William Booth, Esq., 2 Minto Street,	1888
	Dr D. G. Braidwood, Durhambank House, Bonnyrigg,	1889
	Dr Brakenridge, 10 St Colme Street,	1865
35	Dr Byrom Bramwell, 23 Drumsheugh Gardens,	1876
	Dr N. T. Brewis, 59 Queen Street,	1886
	Dr Brown, 1 Bartholomew Road, Kentish Town, London, N.W.,	1884
	Dr J. Graham Brown, 16 Ainslie Place,	1878
	Dr J. Macdonald Brown, 12 South Mansionhouse Road,	1883
40	Dr J. Murdoch Brown, 9 Walker Street,	1885
	Dr Alexander Bruce, 13 Alva Street,	1883
	Dr Robert Bruce, 12 York Place,	1858
	Dr Buist, 1 Clifton Terrace,	1877
	Dr T. M. Burn-Murdoch, 31 Morningside Road,	1886
45	Dr Cadell, 22 Ainslie Place,	1870
	Dr Francis M. Caird, 21 Rutland Street,	1883
	Dr H. L. Calder, 60 Leith Walk,	1884
	Dr W. Watson Campbell, Duns,	1877
	Dr Cappie, 37 Lauriston Place,	1855
50	Dr Edward Carmichael, 8 Mansfield Place,	1887
	Dr J. Carmichael, 22 Northumberland Street,	1870
	Dr C. W. Cathcart, 8 Randolph Crescent,	1883
	Dr T. F. S. Caverhill, 8A Abercromby Place,	1884
	Professor John Chiene, 26 Charlotte Square, *Vice-President*,	1867
55	Dr Church, 36 George Square,	1876
	Dr Clouston, Tipperlinn House, Morningside Place, *Vice-President*,	1861
	Dr A. R. Coldstream, Florence, Italy,	1878
	Dr John Connel, Peebles,	1876
	Dr Cotterill, 23 Walker Street,	1878
60	Dr William Craig, 7 Bruntsfield Place,	1869
	Dr J. R. Crease, 2 Ogle Terrace, South Shields,	1885
	Dr Halliday Croom, 25 Charlotte Square,	1870
	Dr A. S. Cumming, 18 Ainslie Place,	1884
	Dr R. J. B. Cunynghame, 18 Rothesay Place	1868
65	Dr J. B. Darling, 36 South Bruntsfield Place,	1887
	Dr J. M. Dewar, 24 Lauriston Place,	1885
	Dr T. W. Dewar, 4 Stafford Street,	1887
	Dr Archibald Dickson, Hartree House, Biggar,	1871
	Dr George Dickson, 9 India Street,	1884
70	Dr George Dods, 50 Great King Street,	1885
	Dr Halliday Douglas, 30 Melville Street,	1842
	Dr Kenneth M. Douglas, 30 Melville Street,	1888
	Dr William B. Dow, Dunfermline,	1879
	Dr John Duncan, 8 Ainslie Place,	1868
75	Dr Kirk Duncanson, 22 Drumsheugh Gardens,	1871
	Dr H. M. Dunlop, 20 Abercromby Place,	1883
	Dr J. Dunsmure, 53 Queen Street,	1872
	C. H. Fasson, Esq., Dep. Surg.-Gen., Royal Infirmary,	1879
	Dr R. W. Felkin, 20 Alva Street,	1885
80	Dr J. Haig Ferguson, 25 Rutland Street,	1885
	Dr W. A. Finlay, St Helen's, Russell Place, Trinity,	1875
	Dr Andrew Fleming, 8 Napier Road,	1880
	Dr Foulis, 34 Heriot Row,	1875
	Dr F. W. Dyce Fraser, Gorton House, Hawthornden,	1883
85	Dr John Fraser, 19 Strathearn Road,	1878
	Professor Thomas R. Fraser, 13 Drumsheugh Gardens,	1865
	Dr R. Freeland, Broxburn,	1879
	Dr Garland, 35 Charlotte Street, Leith,	1873
	Dr W. Gayton, Bartram Lodge, Fleet Road, Hampstead, London, N.W.,	1886
90	Dr G. A. Gibson, 17 Alva Street,	1880

		Date of Admission.
	Dr James D. Gillespie, 10 Walker Street,	1852
	G. R. Gilruth, Esq., 48 Northumberland Street,	1869
	Dr J. Allan Gray, 107 Ferry Road,	1879
	Professor Greenfield, 7 Heriot Row,	1886
95	Dr David Greig, 38 Coates Gardens,	1854
	Dr W. C. Greig, Australia,	1884
	Dr G. L. Gulland, 6 Randolph Place,	1888
	Dr R. H. Gunning, 12 Addison Crescent, West Kensington, London, W.,	1846
	Dr John Haddon, Marsh House, Canonbie,	1883
100	Dr William Haldane, Viewforth, Bridge of Allan,	1889
	Dr J. W. Hamp, Wolverhampton,	1887
	Professor A. W. Hare, 23 St John Street, Manchester,	1883
	Dr D. Berry Hart, 29 Charlotte Square,	1886
	Dr Henry Hay, 7 Brandon Street,	1884
105	Dr J. Berry Hayeraft, 20 Ann Street,	1889
	Dr John Henderson, 7 John's Place, Leith,	1848
	Dr J. W. B. Hodsdon, 30 Walker Street,	1883
	Dr R. E. Horsley, 46 Heriot Row,	1886
	Dr George Hunter, Linlithgow,	1876
110	Dr W. Hunter, 16 Panton Street, Cambridge,	1887
	Dr Husband, 4 Royal Circus,	1849
	Francis B. Imlach, Esq., 48 Queen Street,	1843
	Dr Robert Inch, Gorebridge,	1887
	Dr W. Wotherspoon Ireland, Prestonpans,	1883
115	Dr James, 44 Melville Street,	1877
	Dr Allan Jamieson, 26 Rutland Street,	1876
	Dr Hugh Jamieson, care of A. W. Hutton, Esq., M.A., 479 Avenida Montesdeoca, Buenos Ayres,	1889
	Dr James Jamieson, 43 George Square,	1877
	Dr G. M. Johnston, 9 Morton Street, Leith,	1888
120	Dr R. M'Kenzie Johnston, 44 Charlotte Square,	1883
	Dr J. Carlyle Johnstone, Melrose Asylum, Melrose,	1882
	Dr J. Keay, Mavisbank House, Polton,	1887
	Dr Keiller, 21 Queen Street,	1845
	Dr Skene Keith, 42 Charles Street, Berkeley Square, London, W.,	1885
125	Dr Thomas Keith, 42 Charles Street, Berkeley Square, London, W.,	1852
	Dr C. Kennedy, 25 Newington Road,	1886
	Dr George Kerr, 6 St Colme Street,	1885
	Dr J. H. A. Laing, 11 Melville Street,	1889
	Dr W. Scott Lang, 1 Leopold Place,	1886
130	Dr Robert Lawson, 24 Mayfield Terrace,	1881
	Dr R. F. C. Leith, 129 Warrender Park Road,	1886
	Dr George Leslie, Falkirk,	1881
	Dr Linton, 60 George Square,	1863
	Dr Littlejohn, 24 Royal Circus,	1853
135	Dr Harvey Littlejohn, 24 Royal Circus,	1889
	Dr Lucas, Dalkeith,	1875
	Dr R. A. Lundie, 35 Warrender Park Road,	1883
	Dr P. M'Bride, 16 Chester Street,	1879
	Dr E. M'Callum, 3 Brandon Street,	1885
140	Dr W. Burn Macdonald, Port Lodge, Dunbar,	1888
	Dr J. M'Fadyean, 9 East Hermitage Place, Leith,	1888
	John M'Gibbon, Esq., 55 Queen Street,	1868
	Dr MacGillivray, 11 Rutland Street,	1877
	Dr G. Mackay, 2A Gilmore Place,	1878
145	Dr George Mackay, 2 Randolph Place,	1887
	Dr G. H. Mackay, Elgin,	1885
	Professor Sir Douglas Maclagan, 28 Heriot Row,	1834
	Dr J. S. M'Laren, 14 Walker Street,	1887

		Date of Admission.
150	Dr P. H. Maclaren, 1 Drumsheugh Gardens,	1868
	Dr Roderick M'Laren, 23 Portland Square, Carlisle,	1882
	Dr J. W. Martin, Gie Appantoo, Axim, Gold Coast, Africa,	1888
	Dr A. Matthew, Corstorphine,	1882
	Dr D. Menzies, 20 Rutland Square,	1878
	Dr W. Menzies, 115 Lothian Road,	1847
155	Dr J. C. Messer, 1 Lansdowne Crescent,	1887
	Dr J. Mill, 178 Ferry Road,	1886
	A. G. Miller, Esq., 7 Coates Crescent, *Vice-President*,	1867
	Dr D. Milligan, 11 Palmerston Place,	1885
	Sir Arthur Mitchell, 34 Drummond Place,	1859
160	Dr Moir, 52 Castle Street,	1836
	Dr Alexander Moir, 30 Buccleuch Place,	1876
	Dr Albert Edward Morison, Hartlepool,	1889
	Dr J. Rutherford Morison, 14 Saville Row, Newcastle-on-Tyne,	1882
	Dr John Mowat, 1 Hope Park Terrace,	1885
165	Dr Claud Muirhead, 30 Charlotte Square,	1866
	Dr R. Milne Murray, 10 Hope Street,	1884
	Dr A. D. Leith Napier, 3 Beaufort Gardens, London,	1879
	Dr T. Goodall Nasmyth, Cowdenbeath, Fife,	1884
	Dr E. F. Neve, Dispensary, Srinagar, Kashmir, N.W. India,	1884
170	Dr H. Newcombe, 5 Dalrymple Crescent,	1883
	Dr P. Orphoot, 113 George Street,	1865
	Dr T. W. Pairman, Te Awamutu, Waipa, Auckland, N.Z.,	1884
	Dr Paterson, 4 Coates Crescent,	1847
	Dr D. Noël Paton, 4 Walker Street,	1885
175	Dr Peddie, 15 Rutland Street,	1842
	Dr J. A. Philip, Rue Victor Hugo, Boulogne-Sur-Mer,	1878
	Dr R. W. Philip, 4 Melville Crescent,	1883
	Dr Playfair, 5 Melville Crescent,	1874
	Dr Edmund Price, 28 Mayfield Road,	1889
180	Dr J. H. Pringle, 5 Livingstone Place,	1886
	Dr S. Hale Puckle, Bishop Castle, Shropshire,	1885
	Dr Rattray, Portobello,	1874
	Dr William Richardson, Bath Lodge, Reading,	1884
	Dr James Ritchie, 14 Charlotte Square, *Secretary*,	1873
185	Dr R. Peel Ritchie, 1 Melville Crescent,	1862
	Dr Argyll Robertson, 18 Charlotte Square,	1861
	Dr Ronaldson, 3 Bruntsfield Terrace,	1877
	Dr J. Maxwell Ross, 112 Gilmore Place,	1882
	Dr Joseph C. Ross, 98 Mosley Street, Manchester,	1884
190	Dr T. Home Ross, 40 York Place,	1888
	Dr S. Rumboll, Belgaum House, Woodhouse Lane,	1887
	Thomas Russell, Esq., Davidson's Mains,	1888
	Dr William Russell, 46 Albany Street,	1884
	Professor Rutherford, 14 Douglas Crescent,	1866
195	Dr F. A. Saunders, Denburn, Crail,	1884
	Dr Thomas R. Scott, Musselburgh,	1884
	Dr Andrew Semple, 10 Forres Street,	1883
	Dr John Shand, 34 Albany Street,	1878
	C. H. E. Sheaf, Esq., Toowoomba, Queensland, Australia,	1871
200	Dr G. F. Shiels, 229 Geary Street, San Francisco,	1887
	Dr W. H. Shirreff, Melbourne, Australia,	1883
	Dr J. Sibbald, 3 St Margaret's Road,	1859
	Professor Simpson, 52 Queen Street,	1859
	Dr A. T. Sloan, 22 Forth Street,	1885
205	Dr Andrew Smart, 20 Charlotte Square,	1865
	Dr G. D. Smith, 146 Ferry Road,	1877
	Dr John Smith, 11 Wemyss Place, *President*,	1856
	Dr John Smith, 16 Townsend Crescent, Kirkcaldy,	1889

		Date of Admission.
	Professor Grainger Stewart, 19 Charlotte Square,	1861
210	Dr J. S. Stewart, 16 Merchiston Terrace,	1885
	Dr W. Stewart, Kirkwall,	1879
	Dr H. J. Stiles, 5 Castle Terrace,	1889
	Dr John Strachan, Dollar,	1867
	Dr James Struthers, 39 Charlotte Street, Leith,	1849
215	Dr J. F. Sturrock, 9 James Place, Broughty Ferry,	1887
	Dr Allan C. Sym, 144 Morningside Road,	1889
	Dr William G. Sym, 58 Queen Street,	1889
	Dr Johnson Symington, 2 Greenhill Park,	1878
	Dr W. Taylor, 12 Melville Street,	1871
220	Dr C. C. Teacher, 16 Newington Road,	1887
	Dr C. H. Thatcher, 13 Albany Street,	1876
	Dr Alexander Thom, Crieff,	1884
	Dr H. A. Thomson, 6A Bruntsfield Place,	1887
	Dr John Thomson, 14 Coates Crescent,	1887
225	Dr J. Stitt Thomson, Dalkeith,	1877
	Dr Francis Troup, 1 Minto Street, *Treasurer*,	1883
	Dr Batty Tuke, 20 Charlotte Square,	1864
	Dr J. Batty Tuke, jr., Balgreen, Murrayfield,	1886
	Professor Sir William Turner, 6 Eton Terrace,	1858
230	Dr R. S. Turner, Keith,	1867
	Dr Underhill, 8 Coates Crescent,	1872
	Dr T. Edgar Underhill, Broomsgrove, Worcestershire,	1885
	Dr D. Wallace, 66 Northumberland Street,	1887
	Dr P. H. Watson, 16 Charlotte Square,	1856
235	Dr W. Watson, Midcalder,	1862
	Dr A. D. Webster, 20 Newington Road,	1883
	Dr Graham Weir, 36 Heriot Row,	1843
	Dr A. B. Whitton, Aberchirder,	1886
	Dr David Wilson, 12 Dean Terrace,	1844
240	Dr J. Lockhart Wilson, Duns,	1888
	J. L. Wilson, Esq., 4 Buccleuch Place,	1883
	Dr T. D. Wilson, 10 Newington Road,	1880
	Dr William Wilson, 2 North Charlotte Street,	1885
	Dr Oswald G. Wood, India,	1886
245	Dr Russell E. Wood, 9 Darnaway Street,	1883
	Dr G. Sims Woodhead, 6 Marchhall Crescent,	1883
	Dr Strethill Wright, 8 St Aidan's Terrace, Claughton, Birkenhead,	1871
	Dr Hamilton Wylie, 1 George Place,	1883
	Dr John Wyllie, 1 Melville Street,	1868
250	Dr James Young, 14 Ainslie Place,	1859
	Dr P. A. Young, 25 Manor Place,	1870
	Dr Ziegler, 47 George Square,	1876

NON-RESIDENT.

	Dr F. W. Barry, *London*,	1878
	Dr Bethune, *Toronto*,	1861
255	Dr J. S. Beveridge, *Edenbridge*,	1861
	Dr J. W. Black, *London*,	1865
	Dr Brodie, *Canterbury*,	1865
	Dr John Brown, *Burnley*,	1878
	Professor Cleland, *Glasgow*,	1864
260	Dr Coghill, *Ventnor*,	1870
	Dr P. M. Deas, *Exeter*,	1868
	Dr J. W. Eastwood, *Darlington*,	1871
	Dr R. B. Finlay, M.P., *Middle Temple, London*,	1864
	Dr Foulis, *Cupar-Fife*,	1859

		Date of Admission.
265	Professor Gamgee, *St-Leonards-on-Sea*,	1863
	Peter Gordon, Esq., *Juniper Green*,	1861
	Dr A. Graham, R.N.,	1853
	Dr Groesbeck, *Cincinnati*,	1875
	Dr Archibald Hall, *Montreal*,	1853
270	Professor D. J. Hamilton, *Aberdeen University*,	1876
	Dr J. H. Hay, *Alloa*,	1880
	Dr Stanley Haynes, *Malvern*,	1864
	Dr J. S. Howden, *Montrose*,	1856
	Dr James Johnston, *London*,	1871
275	Dr George Keith, *Currie*,	1845
	Dr Lowe, *Wimbledon*,	1845
	Dr F. R. Macdonald, *Inveraray*,	1860
	Dr K. N. Macdonald, *Cupar-Fife*,	1880
	Dr John A. Macdougall, *Carlisle*,	1875
280	Dr John Mackay, *Aberfeldy*,	1881
	Professor M'Kendrick, *Glasgow*,	1870
	Dr W. O. Mackenzie, D.I.G.H., *London*,	1845
	Dr T. J. Maclagan, *London*,	1875
	Dr J. Moolman, *Cape of Good Hope*,	1877
285	Dr J. Ivor Murray, *Scarboro'*,	1857
	Dr Andrew Myrtle, *Harrogate*,	1859
	Professor Bell Pettigrew, *St Andrews*,	1873
	Dr Phillippo, *Kingston, Jamaica*,	1860
	Professor W. S. Playfair, *London*,	1857
290	Dr Priestley, *London*,	1854
	Thomas Sheriff, Esq., *Edinburgh*,	1867
	Dr Sinclair, *London*,	1850
	Dr Francis Skae, *Lerwick*,	1864
	Dr T. Skinner, *London*,	1856
295	Dr Van Someren, *Redhill, Surrey*,	1845
	Dr Somerville, *Galashiels*,	1877
	Dr Graham Steell, *Manchester*,	1877
	Professor Stephenson, *Aberdeen*,	1861
	Dr H. R. Storer, *Newport, Rhode Island, U.S.*,	1855
300	Dr Lawson Tait, *Birmingham*,	1870
	Dr Thin, *London*,	1861
	Dr Turnour, *Denbigh*,	1843
	Dr Walter Weir, *London*,	1879
	Dr Yellowlees, *Gartnavel Asylum, Glasgow*,	1862
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TRANSACTIONS

OF

THE MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH,

FOR SESSION LXVIII., 1888-89.

Meeting I.—November 7, 1888.

Dr JOHN SMITH, *President, in the Chair.*

I. ELECTION OF OFFICE-BEARERS.

The following gentlemen were elected office-bearers for the ensuing session:—*President*, Dr John Smith; *Vice-Presidents*, Prof. Chiene, Dr Clouston, Mr A. G. Miller; *Councillors*, Prof. Grainger Stewart, Dr J. Connel, Dr George Leslie, Dr Maxwell Ross, Dr Ireland, Dr Matthew, Dr A. H. F. Barbour, Mr Charles W. Cathcart; *Treasurer*, Dr Francis Troup; *Secretaries*, Dr James Ritchie, 14 Charlotte Square, and Mr F. M. Caird, 21 Rutland Street; *Editor of Transactions*, Dr William Craig, 7 Bruntsfield Place.

II. EXHIBITION OF PATIENTS.

1. *Dr M'Bride* showed—(*a*), A young man suffering from an unusual form of LARYNGEAL NEUROSIS. In the erect position, or when kneeling with his head thrown back, he could only speak in a high falsetto note. The laryngoscopic appearances in this position were those usually seen in the production of falsetto notes. He believed the condition to be due to a defect in the crico-thyroid muscle. This was rendered probable by the fact that electricity and massage applied to it improved the voice, and because when the cricoid and thyroid cartilages were pressed together by the fingers the falsetto note was replaced by a normal voice. (*b*), A

woman who suffered from OSSEOUS CYSTS (containing air) of the middle turbinated bones, causing nasal obstruction. In the one nostril he broke down the cyst, removing part of the walls; in the other he contented himself with opening into the cyst and squeezing the walls flat. The result was that in the latter of the two the cyst again expanded.

2. *Dr Affleck* showed a woman, aged 27, who had recovered from an attack of ALCOHOLIC PARALYSIS after treatment by massage. She was admitted to his wards in the Royal Infirmary on 1st March of the present year, showing the symptoms in an aggravated form,—severe pains in the feet and limbs, together with cramps, remarkable paræsthesiæ, loss or delay of cutaneous sensibility over various areas in the lower extremities and hands, both hands in the condition of wrist-drop, along with complete paraplegia and great atrophy of all the paralyzed muscles. There was also marked muscular hyperæsthesia, and while the skin was insensible to touch, when the muscular mass was laid hold of, the pain was agonizing, and continued after the pressure was removed. The hands showed peculiar athetotic or disorderly movements. There was also marked vaso-motor disturbance and acceleration of the pulse, the rate being 140, and no increment of temperature, due probably to paralysis of the inhibitory fibres of the heart, remarkable hebetude, and loss of memory. The whole case was quite characteristic of the condition, and the history was one of long-continued alcoholic excess. The symptoms had continued from early in January. She was absolutely helpless. There was no visceral paralysis. The electrical reactions were much impaired, and the reaction of degeneration well marked in the lower extremities. The treatment consisted in the entire suspension of the cause, and the administration of good aliment and tonic remedies—iron, quinia, and strychnia, and also the use of bromides to relieve the severe neuralgic and myalgic pains. She was also treated by galvanism and gentle friction with belladonna liniment. For several months there was no change, and preparations were being made for her removal to the Longmore Hospital, when he persuaded his friend and former house-physician, Dr Laing, who was studying treatment by massage, to take her in hand. He began to manipulate individual and groups of muscles, at first very gently, rubbing in the direction of the blood current, moving the joints and the limbs, and getting the patient herself to assist in moving them also. In a short time she began to show marked improvement. The limbs increased in bulk, the skin began to look more healthy. Then she began to move with ease, and now she could walk about with some briskness.

3. *Dr Allan Jamieson* exhibited a case of LUPUS ERYTHEMATOSUS in its more purely erythematous form, resembling in its extensive diffusion one which he had shown to the Society ten years before,

but which was less superficial. Mrs P., 35, lives in Staffordshire, and enjoys very fair health. Six years since the disease appeared on the forehead and on the sides of the face, but entirely faded during an attack of acute rheumatism. It next showed itself on the arms, not on the face; vanished during pregnancy, to return on the arms again after her confinement. It again disappeared when she was laid up with pneumonia, but when she became well it recurred on the arms, and six months ago returned to its original starting-point—the face. The forehead and face, the sides of the neck, upper part of the chest, backs of the hands and forearms, are affected with a symmetrically disposed bright red eruption, dry in character, slightly elevated at the margins, which are well defined, particularly on the arms. On the face the central portions are somewhat rough and scaly, as if the surface had been dusted over with powder. Some faint scars are visible at the sides of the root of the nose. The complaint once invaded the feet, but does not at present implicate these parts.

III. Original Communication.

NOTES ON REPORT OF THE DEPARTMENTAL COMMITTEE APPOINTED TO INQUIRE INTO PLEURO-PNEUMONIA AND TUBERCULOSIS IN THE UNITED KINGDOM.

By Dr Peel Ritchie, Vice-Pres. Med.-Chirurg. Society, Pres. R.C.P. Ed., etc., F.R.S.E.

In submitting to your consideration the following notes on the Report of the Departmental Committee appointed to Inquire into Pleuro-Pneumonia and Tuberculosis, it may be well, first, to briefly recall the association of this Society with this most important inquiry, especially as no reference to the Society's action appears in published Transactions. On the 15th of February 1888, Principal Thomas Walley, M.R.C.V.S., in an elaborate paper on Animal Tuberculosis in relation to Consumption in Man, directed our attention to the relation existing between animal and human tuberculosis. After a very full discussion of the pathological and scientific aspects of the subject, I moved that, as a practical result, it be remitted to the Council to take steps to carry out some of the author's suggestions, by approaching by memorial, on the one hand the legislative authorities, and on the other the municipal, for improved and extended legal enactment was not only necessary, but the local enforcement of such powers as were already possessed called for more rigorous application. Accordingly, a memorial in the following terms was presented by the President in name of this Society to the Marquis of Lothian, Secretary for Scotland, on the 23rd of March 1888:—

"My Lord Marquis,—The Medico-Chirurgical Society of Edinburgh having had the subject of animal tuberculosis in its relation to man under consideration and discussion along with that of other bovine diseases alleged to be prejudicial to the safety of the community, would most respectfully approach your Lordship, in order to express their sense of the serious nature and momentous importance of these matters. The Medico-Chirurgical Society is composed of the medical practitioners of Edinburgh, as well as of other non-resident medical men, and numbers among its members many who are cognisant not only of the evil which has been clearly traced to these sources, but deeply impressed with the necessity for some active measures being adopted for their prevention. The Society would therefore earnestly pray your Lordship to use your influence and the powers with which you are invested to bring into operation the statutes and enactments of the Government towards the prevention of those evils, and, if necessary, for the introduction of additional means for the regulation and supervision of the whole traffic in milk and in butcher's meat supplied to the inhabitants. I have, with the authority of the Medico-Chirurgical Society, the honour to address you, my Lord, on the subject, and in its name to subscribe myself,—Your Lordship's obedient, humble servant,

JOHN SMITH, *President.*"

On receipt of his Lordship's reply of the 16th April, the Secretary, Mr Cathcart, again communicated with him on the 20th. The Departmental Committee of the Privy Council had commenced to take evidence on pleuro-pneumonia and tuberculosis on the 17th April.

"EDINBURGH, 20*th April* 1888.

"My Lord Marquis,—I have the honour to acknowledge, by instruction from the Council of the Medico-Chirurgical Society, the receipt of your Lordship's communication to the Society through their President, Dr John Smith, dated 16th April 1888. I am directed to inform your Lordship that the Council of the Medico-Chirurgical Society beg respectfully to urge, that the object of the inquiry on bovine tuberculosis will be best served by their sending two members to give evidence on the subject. One of them, Dr Henry Littlejohn, 24 Royal Circus, Medical Officer of Health for this city, will be prepared to refer specially to the etiology of the disease, and to its presence in cattle sent for slaughter and kept in dairies. The other, Dr R. Peel Ritchie, 1 Melville Crescent, President R.C.P., a man of wide clinical experience, will be able to give information as to the transmissibility of the disease to the human subject from the lower animals,—I have the honour, etc.,

CHAS. M. CATHCART."

To this a reply, of date 24th April, in the following terms was received:—

"Sir,—I am directed by the Marquis of Lothian to acknowledge the receipt of your letter of the 20th inst., proposing that two members of the Medico-Chirurgical Society of Edinburgh should be nominated to give evidence before the Committee now sitting to inquire into the subject of contagious pleuro-pneumonia of cattle, etc.; and I am to acquaint you that a copy of your communication has been laid before the Privy Council for the information of the Committee,—I am, etc.,

B. W. COCHRAN PATRICK."

In due course the following letter was received by Mr Cathcart from Mr Richard Dawson:—

AGRICULTURAL DEPARTMENT, PRIVY COUNCIL OFFICE. 44 PARLIAMENT STREET, LONDON, S.W. *April 26th*, 1888.

"Dear Sir,—Your letter of April 20th has been forwarded by the Secretary for Scotland to the Privy Council, and laid before the Departmental Committee on Pleuro-Pneumonia and Tuberculosis. I am desired by the Committee to thank you for this communication, and to say that they will have pleasure in hearing the evidence of Dr Henry Littlejohn and Dr R. Peel Ritchie. The Committee think it advisable to take this evidence towards the termination of the inquiry,—I am, etc. RICHARD DAWSON, *Secretary to the Committee.*"

After my perusal of the foregoing correspondence, it appeared to me to be necessary, as I had been nominated one of the representatives of the Society, seeing it was the views of the Society I was to state rather than my own as an individual, that the secretary should intimate to the Fellows—such Fellows as were thought suitable—that it was considered advisable, in the interests of the Society, that they should supply me with the facts, so far as they were at liberty to do so, which tended to support the view that bovine tuberculosis was capable of being transmitted to human beings, either by the milk of affected animals, or by the consumption of their flesh, for the experience of one individual at this early stage of this inquiry was not yet sufficient; and, also, that the combined observation of many was therefore the more necessary, if due effect was to be given to the memorial of the Society. In reply to this appeal, I regret to say there was only the response, that those written to "have no evidence which they are in a position at present to have brought forward."

On 17th May Mr Cathcart intimated he had received a letter from Mr Dawson to the effect, that it had been decided to summon your representatives to give evidence on 29th May. Whilst informing Mr Cathcart that I had written Mr Dawson that I was ready to attend on that day, I requested that a meeting of the Council should be called in order that I might consult as to the evidence it desired to bring before the Departmental Committee. I submitted the views I proposed giving on behalf of the Society. They received the general approval of the Council, and I thereafter wrote Mr Dawson, that "the line of evidence I propose to submit to the Departmental Committee is, 1st, The reasons which led the Society to address the Secretary for Scotland upon the subject of bovine tuberculosis; 2nd, The evidence submitted to the Society; and, 3rd, The necessity for further experimental inquiry being made into the transmission of bovine tuberculosis to man."

At the meeting of Council it was approved that the points regarding—1st, the state of cow-houses, dairies, and milk shops, and the laws concerning them; 2nd, The exposure and seizure of diseased meat in slaughter-houses and markets; 3rd, The existing powers in dealing with diseased meat or milk; and, 4th, The further extension of these powers by enactment of Privy Council or legislation—should be left for the evidence of the other representative of the Society, Dr Littlejohn.

It is not necessary that I should further allude to the memorial to the municipal authorities.

Before considering the Committee's Report as a whole, it may interest you to learn the terms and nature of the evidence given by your representatives.

As my evidence expressed the views of the Society, it was taken first, but as explanatory of the absence of reference to the Contagious Diseases (Animals) Act, I desire to mention that my evidence was taken as that of a physician, and as approaching the subject from the public health side; and that, as expressed by one of the Committee, Prof. Brown, "an Order emanating from the Privy Council under the Contagious Diseases (Animals) Act, must not have any direct reference to the public health, but must refer to the prevention and spreading of the disease amongst the lower animals." It was left, therefore, to Dr Littlejohn to press this point.

My examination commenced by the Chairman asking as to the representations made by the Society to the Secretary of State for Scotland upon bovine tuberculosis and the reasons for doing so. In reply, I stated the facts with which you are already familiar, the reading of the paper by Principal Walley, the discussion that ensued, in the course of which it was evident that his statements had made a profound impression, and a motion had been carried unanimously that the Government and the municipality of Edinburgh should be approached—the latter with regard to the stricter enforcement of legal enactments regarding byres and dairies; the Government, because the subject was one concerning which there was considerable doubt in the minds of many at present, and it could be better examined into if Government, rather than private investigators or individual members of the profession, were to take it up. I was then asked to give a brief summary of the evidence brought before the Society. In doing so I took the opportunity, first of all, on account of the very great difficulty of diagnosing tubercle in the early stages in cattle, to point out the necessity for skilled inspectors. I then spoke of the bacillus of tubercle as the cause of the disease, and showed that most competent authorities held the doctrine that the tuberculosis of bovines and the tuberculosis of the human subject were identical, that the apparent differences in the bacilli were accidental, and due to the differences in the media in which they were developed and the rapidity of their growth. We held that the identity of the bacilli being proved showed that, if there was not a probability of communication, we nearly all agreed there was a very great possibility; and I went on to say that the contagiousness of tubercle had already been declared by other societies, mentioning especially the meetings of the National Veterinary Association in London in 1883 and in Manchester in 1884, and that of the International Medical Congress at Copenhagen in the same year. I further showed that contagion was possible in three ways: first, by the ingestion of tuberculous

meat, of which, however, we had as yet no proof; second, by the milk from tuberculous cows; and, third, by inoculation from sores to sores—that was to say, from sores upon the udders to sores upon the hands of those who handled them. In support of these two latter methods I gave particulars of the cases which Principal Walley laid before the Society, in which consumption, tabes mesenterica, and meningitis occurred in persons partaking of the milk of tuberculous cows, and one in which a boy with sores on his hands was infected from sores on the udder. I also spoke of the impression made on members by Dr Woodhead's observations on the phthisical death-rate in an establishment supplied by a dairy in which he and Professor M'Fadyean found evidence of tuberculosis in the udders of three cows. A question arises as to whether tubercle in the general system of an animal is sufficient to affect the milk, or is it only in cases of tubercular udders that the milk is dangerous? Observation points clearly at present to the latter condition only. I then narrated two cases which occurred in my own practice to support the view that tubercle might be conveyed by cow's milk if a like condition occurred in the human subject. These were the cases of two mothers, previously healthy, who became affected during the suckling of their infants—the one with her first, the other with her third child. The infants, when respectively 4 months and 7 months, died of tubercular meningitis; and the mother in one instance died four and a half, and in the other four months afterwards of pulmonary phthisis. The grounds on which I supposed the infants were infected by the mothers' and not by the cow's milk on which they were latterly fed being the subsequent death of the mothers from tubercular disease. Both mothers, previous to the infants becoming apparently affected, suffered from irritation of the mammary glands; they never suppurated, but there was nodular inflammation of the glands. I was examined at length upon these cases. I next stated that we also felt, as a Society, that the frequency of tubercular disease in infancy requires explanation, and mentioned that in Edinburgh during the last ten years the average death-rate from tuberculosis under 5 years of age has been 6·8 per cent. of the deaths—that is to say, of 44,616 deaths at all ages, 3054 occurred under 5 years of age from tuberculosis. I also stated we have been in the habit in the past of putting these down to hereditary influences. I here gave in a table prepared for me by Dr Littlejohn, showing the number of deaths, and pointed out the frequency with which disease of the glands of the abdomen and inflammation of the brain occur in young children in Edinburgh. I handed in a table, prepared from a larger one of Dr James's, showing the total deaths in Scotland from tabes mesenterica and tubercular meningitis during ten years, 1876–1885, under 5 years of age, prepared from the Registrar-General of Scotland's returns. During that time 7415 deaths occurred from tabes and 13,216 deaths from tubercular menin-

gitis; and "it was found that, with the exception of the first three months at the respective ages, inflammation of the head always caused the larger number of deaths." I continued,—"Now, it occurs to myself that possibly the greater number of inflammations of the brain have been preceded by disease of the glands of the abdomen. One point is this, that in the first three months the tabes is in a larger proportion, but at all other periods between the three months and five years of age tubercular meningitis is the largest number. Tubercular meningitis, according to the return, may thus be regarded as one of infection from tuberculous glands. The glands, therefore, possibly may have become affected from the milk which the children have been fed on (I am merely speaking as a possibility); and the secondary disease, inflammation of the brain, may have thus often resulted." In course of further questioning upon this head I observed,—"Meningitis being much more rapid in its fatal course, presents the more urgent and formidable symptoms, and to it is assigned the cause of death; whilst the tabes, being less evidently active, may be overlooked, or not mentioned, in the certificate of death." I concluded this part of my evidence by stating that,—"So little is the subject known amongst medical men at present, that I interviewed, before coming up to this Committee, between forty and fifty of our leading medical men in Edinburgh, and, with the exception of Dr Woodhead, none of them had seen a case in a human subject which they could say resulted from bovine tuberculosis. They had in several instances had suspicions that such a thing might have occurred; they nearly all agreed as to the possibility of its occurring. Some of them doubted its possibility; but they were all unanimous in this, that the subject was one of such great importance that they thought an inquiry on the part of the Government was necessary to allay the popular mind and the doubts at present existing." I gave it as my opinion that this inquiry should consist of experiments and investigation into the modes of communication of the disease, that it should be extensive in character, and conducted by a Board which should not confine its labours to one part of the country. As to the causes, I thought the conditions the cows are kept in lead to it, and referred to their insanitary state in Edinburgh. The want of exercise in stall-fed cows aided in producing a state of health favourable to development of tubercle. Artificial feeding had, I also thought, some influence, the cows being fed on milk-producing foods rather than upon blood and flesh improvers. In-and-in breeding might act by predisposing to hereditary constitution.

In answer to questions by Prof. Horsley, I said the Society had not considered the evidence of the communication of the disease from a husband to a wife; that though the disease might be introduced by inhalation, we thought it was not so in cattle. Possibly the cow got it in the stall or from the insanitary state of its

surroundings. Cows were apt to lick what had passed from the mouth of another affected animal if it had dried up on the stall in which it was, and in that way the bacilli might be introduced into the cow's system. I had no figures to show the actual connexion between diseased milk or flesh and tabes mesenterica and meningitis, but that it was on these and such points that we were at present so much in need of enlightenment, and that it would be highly advisable for the general sanitary condition of the nation that an inquiry into them should be instituted; that we desired, if possible, to have a check put on the way in which diseased animals at present are exposed for sale, are slaughtered, and are treated, and so strengthen the hands of the veterinary inspectors in getting these animals properly looked after. I considered that a thorough and regular inspection by skilled men was necessary to prevent the spread of the disease among cattle and its transmission to the human individual, and that such inspectors would require special training in the use of the microscope and the detection of the bacilli.

The Chairman and Mr Stirling both put questions as to the phthisical and ordinary death-rates in Edinburgh of late years, to which I replied that the ordinary had diminished, and it was not found that the deaths from phthisis were increasing, but it was one of our difficulties that we did not know when or how long this condition that we were now wanting information about may have been continuing or going on. I stated further that the diminution in the death-rate was largely in consequence of the excellent sanitation of the Medical Officer of Health. I was also asked as to the destruction of the bacilli, and said that in milk properly boiled the bacilli were killed, but I could not speak with certainty as to the spores.

Professor Brown then took up the questioning with inquiry as to my views as to skilled inspectors, but I referred him for information regarding those now engaged at this work to Dr Littlejohn, repeating that they would require to be specially trained in the use of the microscope and the methods of demonstrating bacilli. My examination concluded with questions by the Chairman whether milk should not be supplied from country dairies under proper medical and sanitary conditions rather than from town dairies; but I held, as the introduction of the late epidemic of scarlet fever had been from the country, that I did not see why our town dairies should not be made perfectly capable of supplying good milk.

Dr Littlejohn was then called and examined. After preliminary questions he informed the Committee that in Edinburgh the carcase of an animal affected with tuberculosis was not allowed to be sold except the disease was limited, the glands unaffected, and the flesh on section sound; that the authorities had no data to prove that the use of flesh of animals so affected had any effect on

the public health (but this was a matter for further investigation and experiment); that they had no power to seize tubercular animals in the markets or cowsheds, nor of preventing the milk from such animals being mixed with that from healthy animals. They had powers to seize diseased meat, but they wanted more than that—to take the live animal. He would like that they should have similar powers to those which he believed were enjoyed in England and Ireland, so that their inspectors might take any animals from the public markets which exhibit a suspicious appearance of emaciation, summoning the veterinary inspectors of the corporation, and craving powers from the magistrates to have that animal destroyed. He thought the Greenock people had such powers under a clause in their Act of 1877. He was also of opinion that tuberculosis should be included in the Contagious Diseases (Animals) Act, because this would give them power to prevent the milk of suspected animals being mixed with other milk or sold for human food, and would take from local authorities the excuse that they had no power in dealing with this disease, and as public opinion was being so much directed to this point, it would compel these authorities to more stringently enforce the Dairies, Cowsheds, and Milk-shops Order than they do at present. He was under the impression that tuberculosis was on the increase, and gave figures showing that in Edinburgh in 1877, 51 carcases out of 29,665 total cattle, and during the first five months of 1888, 46 carcases out of 10,756 were condemned. Compensation he spoke of with diffidence, but would be inclined to grant it only where a mistake had been made. He considered the disease was more common among dairy than among ordinary farm stock, but could not say with certainty that tuberculosis could be communicated from the bovine animal to the human subject. That the disease was more common among dairy stock was due, he thought, to the manner in which their town dairies were managed. The ventilation was usually bad; everything was kept in a state of constant damp; the animals tied close up against the wall and close together, without provision for the proper cubic space, and never moved out from the one year's end to the other, until they were taken away for slaughter. He recommended notification, but thought that, having regard to the difficulty of diagnosis, a man should not be punished for not giving notice. He thought it desirable that milk should be examined by the microscope and other means, and would make the examination for the tubercle bacillus a part of the ordinary inspection of the dairy. He did not, however, suggest it as a remedy, unless it was proved to his satisfaction that in every case of tuberculosis they could detect the peculiar bacillus in the milk, which he strongly doubted, because, as far as his own experience had gone, he had only found it in cases where the udder had been markedly affected. Asked as to the proportionate distribution of the disease in the bodies of animals, he said he

had chiefly observed it—first, in the pleura and peritoneum; second, the liver; and third, the glands generally. The lungs were rarely affected, the animal apparently being killed before they became so. He had never observed deposit of tubercle in the muscle of a bovine animal, but had in that of a pig, and had seen it also in the joints and bones, but usually the animals were killed before the disease had become so extensive. He had no evidence of the spread of the disease from one animal to another.

The remainder of Dr Littlejohn's examination had reference to the carrying out of the Milk-shops Order, the temperature necessary for the destruction of the bacillus of tubercle, the status and pay of the inspectors, the necessity for public abattoirs, the inspection of country dairies and the disposal of their milk to middlemen, the dairy companies in England and Edinburgh, and their special private sanitary arrangements. The water supply of Edinburgh dairies was then touched on, and the risk from its not being satisfactory or imperfect in country dairies, concluded his evidence.

Notes on the Report.

The Departmental Committee was appointed to inquire into and report upon the nature and extent of Pleuro-Pneumonia in the United Kingdom, and the effects of Inoculation and other Preventive Measures on that Disease; also to inquire into the nature and extent of Tuberculosis in the United Kingdom, and the means to be adopted to arrest its progress. Of course, as regards the latter, it is from the animal rather than the human side it is considered.

The Committee consisted of seven members, and Mr Richard Dawson was appointed to act as secretary. Mr J. Naper was originally named a member, but being unable to attend, another member was nominated. The following were the members:—Jacob Wilson, Esq., was elected chairman; Sir G. Macpherson Grant, Bart.; J. Bowen Jones, Esq.; P. Stirling, Esq.; Victor Horsley, Esq., F.R.C.S.; Professor Brown, C.B., of the Brown Institute; Lord Cloncurry. They were appointed on 16th April 1888 by Lord Cranbrook.

The Committee commenced taking evidence on the 17th April, and continued its meetings from time to time up to the 4th of June. During that interval it sat on seventeen days, and examined 44 witnesses. Of these, 13 represented England, 16 were from Scotland, 6 from Ireland, 6 were Colonial—from the Cape Colony, New South Wales, Victoria, and Natal. One of these had, however, been previously resident in New Zealand. Two were officials connected with the Agricultural Department of the Privy Council; whilst one was foreign, a Government veterinary surgeon from the Hague, Holland. Arranged according to designation:—

1. Justice of Peace, ...	1
2. Representative of Local Authority, .	1

3.	Chief Constable,	1
4.	Officers of Health,	2
5.	Inspectors Agricultural Department P. C.,	2
6.	Veterinary Inspectors,	4
7.	„ Professors,	2
8.	„ Surgeons, British, 4 „ „ Foreign, 1	5
9.	Medical Experimenter for Local Government Board,	1
10.	Physician,	1
11.	President Shorthorn Society,	1
12.	Landowner,	1
13.	Stockowners, Colonial,	6
14.	Stock-dealers, 2 Stock-dealer and farmer, 1	3
15.	Farmers,	8
16.	Dairymen and cowkeepers,	3
17.	Superintendents, Dairy Co., 1 Model Farm, 1	2

The Report consists of two parts: 1. The Report; 2. The Evidence.

The subjects remitted to the Committee were—Pleuro-Pneumonia: its Nature, Extent, Effects of Inoculation, and other Preventive Measures. This forms the first part of the Report, with which, as a medical society, we have nothing to do. Tuberculosis (Animal): its Nature and Extent in the United Kingdom, and the Means to be Adopted to Arrest its Progress, forms the second object of the Committee's inquiry, and it is with it we, as a Society, are interested. Of the 44 witnesses, 27 gave information of varying importance on tuberculosis, 17 upon pleuro-pneumonia alone.

The Report on Tuberculosis considers, first, the Nature of the Disease.

The nomenclature introduces the subject of the Report, and after an examination of the various tubercular diseases, it proceeds,—"We now know for certain that they are all forms of one and the same process, and caused by a microbe," which "forms the poison or virus of the disease."

The discovery that inoculation of the virus into the lower animals is capable of producing the malady is assigned to Klencke in 1843, but that Villemin in 1865 first placed it on a firm basis; "the nature of the poison itself remained unknown until it was discovered by Koch, in 1881, to be a rod-shaped microbe." The microbe is then defined, the greater vitality of the spores is dwelt upon, and the importance of it and the viability of the rods are pointed out,—"Since, if the mucus, or saliva, or expectoration of an animal or human being suffering from tuberculosis be dropped upon the ground, flooring, or furniture of a room or shed, it is

obvious that such secretions are, in proportion to the effect which exposure at the temperature of the air and drying may have in destroying the organisms and their spores, a source of danger to other animals or human beings who may accidentally take up the poison."

The effect of temperature is next reviewed—that most favourable to the growth of the microbe being the ordinary heat of a warm-blooded animal, from about 98°·5 to 100°·5 Fahrenheit. At 82° Fahr. growth ceases, but this degree does not kill the microbe. If "kept at a temperature of about 107°·5 Fahr. for several weeks, the organism gradually becomes exhausted, and dies." The effect of drying upon the microbes is mentioned; few resist desiccation; but from experiments made upon expectoration containing bacilli, it has been proved that such expectorations may be kept for several months successively dried and moistened, and then, when inoculated into animals, the bacilli are found not to have been killed, as "they have actively produced the disease."

It is pointed out that tubercular discharges in this climate may remain virulent for a long time; and stalls and sheds, unless thoroughly cleansed, be a source of danger.

The order of liability of domesticated animals is given thus:—1. Man stands first; then 2, milch cows; 3, fowls; 4, rodents; 5, pigs; 6, goats; 7, sheep; 8, horses; 9, carnivora, *i.e.*, dogs, cats, etc. (very rarely). "From this," the Report adds, "it appears that the organism grows most readily in those animals which are omnivorous and herbivorous,"—a summing up which, perhaps, requires a little modification. From the evidence, and also subsequently in the Report, a distinction as to frequency of tuberculosis is shown to exist between stall-fed cows and field cattle, but in the foregoing list this is not alluded to. The greater liability of the female sex and young animals to become affected is also mentioned.

Five predisposing conditions are next reviewed:—

1. *Starvation*, as causing degeneration of tissue and diminishing resistance to microbe growth.

2. *Deficiency of oxygen by bad ventilation.* This, whilst predisposing the one animal, also favours the transmission of the virus to another.

3. *Exhausting secretions*, e.g., *prolonged lactation.* "The constant loss of the fat, *albumen*, and salts contained in the milk" produce "those degenerative changes which reduce the vital resistance of the animal."

4. Possibly heredity (afterwards referred to), an influence "attributed by some to the transmission from parent to offspring, not of the actual virus, but of a condition of tissue which is peculiarly favourable to the development of that organism."

5. *Certain foods* (*asserted, but very doubtful*). "Some foods, *i.e.*, grains, etc., have been imagined to favour the occurrence of tuberculosis, but this is extremely problematical." Although this is

the deliverance of the Committee, I remain of the opinion I expressed, that, looking to the drain from the milch cow's system, feeding the animal upon the kinds of food which favour the flow of milk, unless attention is paid to also supplying a compensating amount of blood and flesh producing food, cannot but aid in the production of such degenerative changes as to reduce the vital resistance of the animal.

The modes in which the virus or microbe enters the body have, it is stated, been proved to be the following:—

1. *Inhalation,* into the air passages and lungs.
2. *Swallowing,* into the alimentary or digestive system.
3. *Direct introduction,* into the subcutaneous or submucous tissue by means of a scratch or cut or sore in the skin or mucous membrane.

It is also supposed to be directly transmitted by 4. *Heredity.*

Inhalation.—From "the fact that the signs of disease are most commonly found in the lungs, inhalation would appear to be the commonest way in which the disease is contracted." The result of comparative experiments has tested this; and the Report continues:—"The results of these experiments have been almost invariably positive, the animals breathing such infected air rapidly succumbing to the disease." For my own part, I can readily believe the results of experiments so conducted; but, unless the infecting animal's lungs were in a very advanced stage of disease, I cannot say that I can quite accept this statement as altogether correct. The deduction is that "cohabitation of the diseased and healthy animals is a fertile source of spread of the malady."

Swallowing is also supported by numerous experiments. The virus in mucus, saliva, milk, portions of "diseased tissues and cultures of the bacilli, have been swallowed by calves, pigs, sheep, rodents, fowls, etc., with the effect that the disease has fatally followed the ingestion." It is added that "it is evident the digestive fluids do not necessarily exert an injurious influence upon the poisonous bacilli." I would here refer to the evidence (p. 256) of Mr A. Lingard for account of the case described by Dr Lamallerée in the *Gazette Médicale* of 15th Aug. 1883, in which tuberculosis was not only transmitted from man to fowls by their consuming the expectoration of a phthisical woman (who, it is presumed, had been infected by her phthisical husband), but also was transmitted from them to a previously healthy woman, who within a period of three months had eaten ten or twelve of the fowls which had died of tuberculosis.

Direct introduction.—The microbes make their way to the glands; these become diseased; the microbes pass by the lymphatics to the veins, and the virus is distributed through the body. "Undoubted instances have been laid before us of such inoculation occurring," "and one or two stated in the evidence in which a bull has given the disease to cows; and the converse has also occurred, namely, that a bull has contracted the disease from cows."

Heredity.—The predisposition through the "tissues of one particular breed or race" being "favourably disposed to nourish the tubercle bacillus" has been already referred to; but "whether the bacillus is actually contained in the ovum or spermatozoon" is considered. The statements by Baumgarten, that he has "observed the bacillus in the rabbit within the ovum," and that other observers have seen them mingled with active spermatozoa, are given. In support of the transmission theory, the case of Professor Johne of Dresden is referred to, in which the infected intrauterine 7 months' calf of a tubercular cow is mentioned, and it is remarked, "Similar intrauterine infection has been shown to have been more than probable in the human being;" but it is also added, "Against this view of the infection of the ovum and embryo it has been suggested that the disease-producing influence of the bacillus would prevent the ovum arriving at maturity." Stock breeders have discovered the risk "of breeding from tubercular stock." In-breeding, as giving rise to tuberculosis, is held, as was to be expected, to be erroneous; but as predisposing to infection, if the virus is introduced into the herd, is held as probable.

The Mode of Attack and Distribution of the Disease within the Body.—The disease may attack the body of an animal as *acute* or *general* or *miliary tuberculosis*, or more chronically, in the first instance, as *local tuberculosis*, becoming by means of the lymphatic glands subsequently distributed over the body. The local is held to be more common in man than in the lower animals; and the Report continues,—"The distribution of the disease in the body is difficult to connect with any special mode of introduction of the virus, save, perhaps, inhalation." This is to me the more difficult to accept when the next paragraph states,—"Undoubtedly in cattle the lungs and pleuræ and the serous membranes generally are the favourite seats of the malady, any and each of the other organs being occasionally affected." In pigs, the cervical glands; in rodents, the spleen, liver, lungs, and bones; in fowls, the nose, mouth, and spleen; in horses, the glands; and in man, the glands, the lungs, the joints, and the nervous system—are stated to be the commonly affected parts.

As the tubercle bacillus appears to grow best where the circulation is least vigorous, it is held that to this is due the predilection for the lungs, spleen, and joints. Mr Lingard in his evidence states that he found the bacilli at an early period of infection in the marrow of the bones (shown also by Professor M'Call), and before other sign of their presence was observed. This is mentioned in connexion with the question of the use of tubercular meat as food. Evidence was given that though rarely the flesh may be affected, and that ordinary cooking may not be sufficient to destroy the bacilli, for they may also be in the blood, and that, therefore, the chance of their presence "is too probable to ever allow the flesh

of a tubercular animal being used for food under any circumstances, either for man or the lower animals."

The appearance of tubercles, in the tissues, and according to rate of increase and the changes the nodules may undergo, are then described. The presence of bacilli in the secretions from diseased organs is next considered; and the Report continues,—"Of these it is obvious that the fact of milk being infected is of primary importance to the health of both animals and of men, since milk has been proved both to contain the bacilli and to infect the lower animals, *e.g.*, calves, pigs, etc.; while, unfortunately, it is becoming abundantly clear that by the same method of transmission of the virus the disease is communicated to the human being." It will be observed that the question as to whether the udder is the part affected or not is not entertained. If the cow is tubercular in the view of the Committee, both its flesh and its milk are to be regarded as unfit for human or animal consumption. One of the points calling for further experimental observation, the infectivity or non-infectivity of milk from a cow without udder lesions, is thus set aside, but it is a question of much importance still unsettled. In my statement I took the view that it was only when there was udder lesion that the Society had *evidence* of evil result.

The general symptoms of tuberculosis, and the want of them in local slow-growing cases, leading to difficulty of recognition, are touched on, and the characters of the symptoms ultimately, are described, and the evidence to be gained from physical examination of the chest; and this section concludes with the statement,—"The disease in the lower animals always terminates fatally."

Frequency of Proportionate Occurrence among Animals and Men.—The calculated death-rate from the various processes set up by the tubercular bacillus is stated from the statistics of the registers of different countries to be "10 to 14 per cent. of all deaths among human beings." In Paisley it is said the death-rate has been as high as 17·5. In Edinburgh the percentage of animals killed and found tubercular is very low; yet, according to the last report of the Registrar-General for Scotland, the deaths from phthisis alone in Edinburgh were 11·8 per cent. of the total deaths, and from all tubercular diseases 16·4. By the same returns for Scotland the deaths from phthisis number 10·75, and from tubercular diseases 14·55, which is above the maximum average given above. Could we obtain accurate returns of the tubercular cattle killed, it would be interesting to trace their relation to the human tubercular death-rate; and I hope to be able to follow up this subject, now that my attention has been called to it. Amongst animals the proportionate occurrence has not been so clearly made out.

In Dublin the animals slaughtered were at the rate of 4·9 per cent.; and from the evidence the rate varies from 50 per cent. near

Glasgow (Q. 4262); 37·5 per cent. Liberton, near Edinburgh (Q. 7620); 30 per cent. in Paisley and Glasgow stock (Q. 5371); 25 per cent. Ayrshire dairy cows (Q. 835); 4·5 per cent. Victoria (Q. 5582); 3·5 per cent. Lanarkshire (Q. 5360); to ·2 per cent. of all animals, cows, oxen, etc., killed at Edinburgh (Q. 7684)—that is, 97 condemned carcases in Edinburgh out of 40,421 animals killed. It would appear to me that the percentages are given upon results from too small numbers—the 50 per cent. statement being based on the deaths of 13 out of 25 milch cows.

In Germany the proportion among cattle slaughtered appears to vary, from the Report, from 1·5 to 20 per cent., according to district. These variations are explained by the Committee thus: The low percentages are those for open-air fed herds, the high death-rates among dairy cattle cohabiting in sheds.

The frequency of tuberculosis amongst fowls has also attention directed to it as not being generally known, and that both observation and experiment have shown that the fowl contracts the disease from man by swallowing the expectorated bacilli, and again forms a vehicle for its transmission to man and the lower animals.

Several authorities consider the disease amongst cattle is on the increase, but the Committee consider this is doubtful; for probably the apparent increase may be due to better recognition, and that better hygiene has diminished the tubercular death-rate.

The Report next addresses itself to the question of

Remedial Measures.

It says two points are to be borne in mind in the consideration: 1. "That the disease can be transmitted *to* man from the lower animals, and *from* man to the lower animals, by one or other of the methods which we have already discussed, and especially by the ingestion of tubercular diseased meat or milk." The last clause has, of course, reference only to the transmission of the disease from animal to man, or from animal to animal. 2. "That it spreads from animal to animal."

After pointing out that the first of these is usually dealt with under the Public Health Act, the difficulty in dealing with tuberculosis is that "not only is the disease communicated from animals to man, but also from man to animals." Legislation, therefore, which protects cattle from tuberculosis must also prevent its communication to man.

As curative treatment is so unsatisfactory, the Report indicates "that legislation must follow the two lines of—

A. Prevention; B. Extirpation."

Under *Preventive Measures* are reviewed *the improved hygiene of cattle sheds*, etc., as regards ventilation, water supply, disinfection of stalls, etc. It points out that although this has been partly

met by the Dairy and Milk-shops Order, that its administration is imperfect, and "we would suggest that it should be much more stringently enforced, and that veterinary inspectors should be given more extended powers of entry into all places where animals are kept." The isolation of suspected animals, and prevention of the flesh or milk of diseased animals being given as food to others, and restriction as to fodder, litter, and water, are insisted on.

In England and Ireland, although the medical officer of health or inspector of nuisances has power to seize diseased animals in open market, "yet such seizure is rarely performed;" whilst the veterinary inspector has no power to prevent the sale or to order their slaughter, since tuberculosis is not included in the Contagious Diseases (Animals) Act, 1878. "The Committee are very strongly of opinion that power should be given to veterinary inspectors to seize 'wasters' and 'mincers' in fairs, markets, or in transit."

The risk of the disease being imported the Committee does not consider great whilst the regulations as to slaughtering and keeping in quarantine at port continue; but as there may be danger in importation from countries exempted from slaughtering, the present rules are incomplete. Another difficulty arises from the failure of veterinary surgeons to detect the disease in the early stages; and the Committee conclude there could have been no proper veterinary examination in the case of animals found extensively diseased after death, but believed to be sound previously.

Whilst the Report is satisfied with stating that "it is highly desirable that breeders should, in their own as well as in the public interest, discontinue breeding from tuberculous stock," Professor Horsley, in a Supplementary Report, states that, in his opinion, "the act of wittingly breeding from animals so affected should be made 'an indictable offence.'" Prosecutions might arise from the "present state of want of knowledge" "of the early symptoms and physical signs" among cattle-owners, and even veterinary surgeons. He thinks that vexatious prosecutions would be few, and that as each would be tried "before district magistrates on its own merits," the objection would not be of much force.

This brings us to the climax of the Report, Extirpation.

Extirpation.—The Committee are of opinion that tuberculosis should be included in the Contagious Diseases (Animals) Acts, so as to provide:—"For the slaughter of diseased animals when found diseased on the owner's premises." "For the payment of compensation for the slaughter of such animals." "For the seizure and slaughter of diseased animals exposed in fairs, markets, etc., and during transit." "For the seizure and slaughter of diseased foreign animals at the place of landing in this country."

On the plea that tuberculosis may exist without "sufficient outward evidence to enable the owner to detect it," and its growth is so slow "that non-notification of its existence, even in a large number of cases, would do little to nullify the stamping-out effect

of the Act of 1878," notification, the Report decides, should not be compulsory. Professor Horsley, again, differs from this conclusion, and says "that deliberate non-notification should be punished cannot be doubted by any one." He then refers to the objections, and continues,—"As, however, I consider that these objections have been already shown to have no weight, I recommend that both the forbiddal of the breeding from diseased animals and the notification of the disease should be included in any legislation for tuberculosis."

After all the evidence laid before the Committee, the views it has taken of the spread of tuberculosis by the breath of affected animals, and the opinion expressed regarding the presence of tubercle in animals presumed to be healthy, that they had not been submitted to proper veterinary examination, it is somewhat surprising that notification should not be made compulsory. As Professor Horsley states, "Since it is clear that unless the veterinary inspectors or authorities receive information of occurrence of diseases, it is impossible to insure the thorough carrying out of the provisions of the Contagious Diseases (Animals) Act;" and with this I agree. And as we have taken so prominent a position in this question, it seems to me that when the next stage is reached we ought again to step forward and petition in favour of the views expressed by Professor Horsley being made imperative. They both more strongly enforce the action to be taken for the extirpation of tuberculosis in cattle.

The Report considers that inspectors should have the same powers regarding animals suffering from tuberculosis as from pleuro-pneumonia. The Committee are of opinion that the slaughter of diseased animals would go far to stamp out tuberculosis; though, doubtless, owing to heredity, the process would be gradual. Another argument in favour of slaughter is the frequency with which pleuro-pneumonia and tuberculosis co-exist, and are mistaken the one for the other.

Some of the arguments, it must be confessed, seem to imply that veterinary education does not sufficiently progress; but probably what the Committee feel is, that often these animals are not submitted to veterinary inspection till they are exposed or after slaughter, and hence arises the difficulty. Seeing, however, if the Report is adopted and carried out by the Privy Council, the future of tuberculosis is reduced to a question of *diagnosis*, the improved instruction of veterinary surgeons and inspectors must follow, and more allowance in the recommendations should be made for the improvement of veterinary accuracy.

The last clauses of the Report consider the compensation to be given the owner for the compulsory slaughter of his animal, but that is not a question which concerns us professionally.

The Committee conclude by expressing their belief that, if their "recommendations be firmly carried out, pleuro-pneumonia may

within a moderate period be exterminated in this country;" "and although we cannot dare to indulge in such sanguine expectations with regard to tuberculosis, we still venture to hope that much may be done to reduce its extent, and to minimize a disease so dangerous alike to animals and to mankind."

President and Gentlemen, I have concluded my review of this Report. You will doubtless agree with me in regarding it as a very able one, and, further, that its conclusion as to Remedial Measures could be no other than it is. You must remember that the Committee were appointed for practical rather than scientific ends, and that, as expressed by Professor Brown (7644, 5), "under our Acts (Contagious Diseases Animals) we have absolutely no power whatever to deal with the disease solely on the ground that we might take measures to prevent its extension to human beings." "We are bound to show that our measures have reference mainly to the prevention of the extension of the disease among the lower animals."

The evidence showed that tuberculosis was contagious, that it could be transmitted from one animal to another, from man to animals, and, although seemingly of less importance to this Committee after the opinion I have quoted, from animals to man. You sent your two delegates to give evidence as to your views. I endeavoured to do my part, which was, if I may say so, to express the more scientific side of the Society's views. I supported the identity of the disease in animals and man, its transmissibility, and the scientific doubts existing in the minds of many, and the necessity for experiments conducted on a large or extended scale, such as only could be done by Imperial action. I viewed the question from the human side.

Your other representative viewed it from the animal or, in this case, the practical side. In order to deal with the diseased animals, he supported the other views the Society brought forward, that the animal suffering from tuberculosis was injurious to its fellows, as well as to humanity, and pleaded that the disease should, for various practical reasons, be classed among the contagious diseases of animals.

The scientific inquiry for the Committee's purpose had gone far enough. The further points we wanted to have cleared up by extended investigation, in their eyes, were unnecessary. If the milk was injurious, whether it came from a tuberculous udder or a tuberculous cow, was of no consequence from their point of view. Whether the flesh of an animal was capable of producing disease or not, if tubercle existed in other parts of the body, was of no consequence. To trace the effects of milk from a cow found to be tubercular did not come within their range of inquiry. If butter or cheese made from the milk of a tubercular Ayrshire cow was the means of disseminating tubercle was not the question of im-

portance with them. That a bull with tubercular disease could directly convey disease to the cow, and that the cow, on the other hand, with a tubercular uterus could convey disease to the bull; that a cow with tubercular uterus was unfit for breeding; that cohabitation in an insanitary byre was injurious; that pigs fed on tubercular milk had the disease produced in them—were of chief importance. These things were held proved; and without going into the nicer questions I have hinted at as subjects for further experimental research, they put an end to all questions, for if tubercle exists in the animal, the bacilli are probably in the blood, and they therefore declare a tubercular animal, whether locally or generally affected, to be dangerous both in its living and its dead state, and stop further inquiry after the presence of tubercle is proved. For practical sanitary purposes we cannot but approve, but as scientific inquirers we may possibly regret that so many details are left still unsatisfied.

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The President said that the Society was much indebted to Dr Ritchie not only for the paper just read, but for the services there alluded to, and which had been rendered the Society by Dr Ritchie in conjunction with so able an authority as Dr Littlejohn, both of whom had acted as its representatives before the committee in London. In doing so he wished to state at this, the first meeting of the session, that while a paper of this kind might be considered as partaking to a considerable extent of the nature of a report, and consequently, perhaps, be regarded as not quite so apposite for discussion, there were others every session, of which a due advantage did not seem to be taken by the Society, either in the attendance of its older, or the work of its younger members. It was very important that an interchange of opinions should take place at these meetings; that every member should offer what information he could; that original views should be promulgated, and mutual instruction be promoted in every possible manner. Now these objects could not be adequately or properly attained where the discussion devolved, meeting after meeting, upon the same few members; and he would earnestly impress upon those attending the Society the necessity and the propriety of their coming prepared to offer such remarks, or make such suggestions, or put such questions without wandering from the point, as occurred to them after the reading of papers, and that without hesitation. It was probable that more time would now be afforded for such discussions or conversational debates, as it was under the consideration of the Council at present to require very lengthy papers to be condensed in their being read before the Society. Any remarks or discussion upon patients introduced or specimens exhibited must be, and had always been, necessarily limited; but he thought it most desirable that the number of

speakers upon the ordinary communications should be much more generally distributed among the members than was at present the case.

Meeting II.—December 5, 1888.

Dr John Smith, *President, in the Chair.*

I. Election of New Members.

The following gentlemen were elected ordinary members of the Society:—George P. Boddie, M.B.; Kenneth Mackinnon Douglas, M.B., C.M.; G. Lovell Gulland, M.B., M.A., B.Sc., M.R.C.P. Ed.; W. Burn Macdonald, M.A., M.B.; James W. Martin, M.D.

II. Exhibition of Patient.

Professor Grainger Stewart showed a case of gout with numerous tophaceous deposits in the skin. The patient was a man, aged 58, who had suffered from gout for twenty-three years, the hands having gradually become so deformed as to make work impossible for a long time past. Till lately there was no manifestation except in the joints and skin, but albuminuria had now appeared, and hyaline casts were sometimes seen. Probably there was no case on record with so many deposits in the skin. The case was well known in the London hospitals, and has been referred to by Dr Pye Smith in the last edition of Hilton Fagge's book. The family history could be traced back at least a hundred years on both father and mother's sides, and there was absolutely no case of gout on either. Though he was not an abstainer he was not a self-indulgent man, and his personal habits did not give them any clue to the cause of the early and severe development of the disease.

III. Exhibition of Instrument.

Dr James Foulis showed a new form of aspirator syringe.

IV. Exhibition of Pathological Specimens.

1. *Mr Scott Lang* showed for Mr Joseph Bell a carcinoma of the mamma associated with Paget's disease of the nipple, and a cast by Mr Cathcart showing the external appearance of the breast immediately after removal and before a section was made. A microscopical specimen was also shown. Under the microscope the condition of the breast was seen to be very similar to that recently described by Sir Spencer Wells as alveolar cancer, or otherwise as duct-cancer. There was a history of heredity in the

case, Mr Bell having seen in consultation the lady's aunt for a precisely similar condition, which had come on late in life and lasted for many years. No operation was possible in her case, on account of advanced age and frequent attacks of hemiplegia with aphasia. The eczema appeared to have begun in the areola around the nipple, and, according to Sir Spencer Wells, was connected with the orifices of the sweat glands in the areola. In this case the patient made a rapid recovery, and the prognosis in such cases was favourable, according to Sir Spencer Wells in his recent lectures on cancer and cancerous diseases.

2. *Prof. Chiene* showed specimens recently added to his class museum. (*a.*) The first of these consisted of a series of URINARY CALCULI from four patients—three male and one female. One of the male patients was a boy aged $4\frac{1}{2}$ years, the point of interest in his case being an illustration of the risk during operation of tearing the membranous urethra and pushing the bladder up into the abdomen. As Mr Chiene introduced his finger, he felt the urethra beginning to give. He therefore desisted, and incised the neck of the bladder more freely. The other two were adults, aged 52 and 60 respectively. In the former, there was rigidity of the prostate requiring free division, which resulted in considerable delay in the healing of the sinus. In the latter, the prostate was so much enlarged that the finger could not reach the bladder. In the female, aged 20, the trouble was due to the introduction of a foreign body. Dilatation of the urethra was found insufficient, and an incision had to be made into the floor of the urethra before the stone could be removed. Neither incontinence nor retention followed the operation. (*b.*) TUMOURS. Of these, three were exostoses—one of the lower end of the fibula in a patient aged 30, and ascribed to a strain twelve years before, another of the femur in a patient aged 18, and the third from the posterior border of the scapula of a patient aged 15. The fourth was a cartilaginous tumour, growing from the perichondrium of the fourth left rib at the junction of the cartilage with the bone. It was removed by crescentic incision at the lowest part of the tumour, by which the skin and pectoral muscle could be easily raised from the surface of the growth. As the attachments could not be made out easily, the tumour was removed in slices. A cast showed the appearances before removal. Fifth, two bursal tumours, one from the right, the other from the left patella of the same patient. The right had existed for thirteen years, the left for six. Sixth, a myeloid sarcoma of the lower jaw of a man aged 59. It had existed for nine months. Half of the jaw was removed immediately after a preliminary tracheotomy. Four days after, symptoms of pneumonia appeared; eleven days after the occurrence of the pneumonia, there was evidence of gangrene of the lung, and death took place in forty-eight hours. Prof. Chiene was inclined to think that

there must have been some regurgitation of food from the stomach into the trachea which would account for the result. Seventh, a fibro-cellular tumour from a patient aged 25. It was situated below the auricle, and had first been observed ten or twelve years before. (*c.*) TUBERCULAR TESTES, removed on separate occasions from the same patient (aged 30). The left was first affected after a blow. Five months after its removal he returned with the epididymis of the right enlarged and diseased. (*d.*) A BRAIN with an abscess in the frontal lobe. The patient was a boy aged 13. Four months before admission he was hit on the forehead by a snowball. An abscess formed, which was lanced and healed up. Three months later, after a cold, the swelling returned, and was opened the day before admission into the Hospital. The sinus was explored, and an exfoliation removed from the external angular process of the frontal bone. The pulse was rather slow, but, beyond complaining of headache on the tenth and eleventh days after admission, he showed no symptoms of brain mischief. On the 21st day he ate a quantity of cheese brought in by a relative. This was followed by vomiting and pain in the stomach. The following day he complained of pain under the chin; in the afternoon he was screaming loudly and complaining of pain in the neck and frontal region. Meningitis was then diagnosed. He died the same night, and at the post-mortem examination a subdural abscess, about the size of a walnut, was found in the frontal lobe. (*e.*) ASTRAGALUS and FOOT showing the perforating ulcer of locomotor ataxy. The astragalus was lying bare, and was lifted out when the patient was admitted. (*f.*) A specimen showing the effect of operation for RADICAL CURE OF INGUINAL HERNIA. The hernia had been reduced four days before admission into hospital; the symptoms of strangulation, however, continued, and the hernia was therefore cut down upon. It was then found that reduction *en bloc* had occurred, the constriction was divided, and the radical operation performed. The patient was doing well till, on the twenty-third day after operation, he was seized with angina pectoris, and died suddenly. The specimen, obtained post-mortem, showed closure and dimpling opposite the internal abdominal opening. (*g.*) HAMMER-TOE, showing contraction of the fibrous structures, as well as of the flexor tendons. (*h.*) A couple of PEAS. A stout middle-aged woman put these in her mouth when feeding pigeons. One of them passed down "the wrong way." She brought the other with her to hospital to show it. There was some difficulty of breathing for twenty-four hours after the accident. She was kept in bed, and a pneumonic-like sputum appeared, but no physical signs. This, as well as the cough, passed off, and on the twelfth day she was allowed to go home. On going upstairs she coughed out the pea which had lain in her bronchus for twelve days. (*i.*) A discoidal-shaped WHISTLE, larger than a halfpenny, which had lain in a child's intestinal canal for

twelve days and then passed per anum. (*j.*) FRAGMENTS OF BONE removed from the lower jaw by trephining. The operation was performed for resection of portions of the lingual and inferior dental nerves to relieve the pain of an epitheliomatous ulcer of the tongue. Forty-eight hours after the operation there was considerable salivation and bleeding from the ulcer, due apparently to irritation at the peripheral end of the divided chorda tympani inhibitory and secretory fibres of the lingual nerve. So far as the relief of pain went, it had been a success. (*k.*) Microscopic sections of the fibro-cellular tumour, a healing ulcer, and a hard chancre were also shown.

3. *Dr J. M. Cotterill* showed a perforated VERMIFORM APPENDIX.

V. ORIGINAL COMMUNICATION.

CASE OF GANGRENE OF THE TRANSVERSE COLON IN AN UMBILICAL HERNIA; REMOVAL OF 22 INCHES OF GUT; SUTURE OF THE INTESTINE: RECOVERY.

By J. M. COTTERILL, M.B., F.R.C.S. Ed., Assistant Surgeon, Edinburgh Royal Infirmary.

IN the spring of the present year, I mentioned at one of the meetings of the Medico-Chirurgical Society the case of a patient from whom I had removed a large piece of gangrenous transverse colon. The patient recovered from that operation with an artificial anus at the umbilicus, and I said that I hoped to complete the cure by some further operation, and report the case more fully at a later date. Accordingly, this evening I propose to read a short history of the case from its commencement, and to discuss briefly such points of interest as suggest themselves.

Mrs R., aged 38, was admitted from the Maternity Hospital into Ward VIII. of the Royal Infirmary on 14th April suffering from umbilical hernia. The previous history of the case was to the effect that the patient had suffered for seven years from an umbilical hernia of large size, which, however, had never given any trouble till ten days before admission. At that time the patient reports that she took a long walk, that the streets were slippery, and that she fancied she had strained herself by recovering herself in slipping. When she reached home the rupture was swollen and painful. Fomentations were applied, but as in a week's time she was no better she sent for her medical attendant. At this time Mrs R. was seven months pregnant, and as no success followed the attempts at taxis, a firm binder, which the patient was in the habit of wearing, was reapplied, and she was sent to the Maternity Hospital, as it was feared she was about to miscarry. The patient herself was under the impression that the child had been dead three or four days. At the Maternity Hospital no improvement took place, so she was transferred to Professor Annandale's Wards

in the Infirmary, of which I happened to be in charge at the time.

On admission the following notes were taken:—The patient is a strong-looking woman, but enormously fat, being some eighteen stone weight. At the summit of a large fat abdomen, distended by a uterus seven months pregnant, is a bright-red, angry looking, brawny swelling about fourteen inches in diameter, and projecting some four inches from the general outline of the abdominal profile. The patient is anxious and in pain, and the face is pinched. Pulse, 110; temperature, 100°·2. No vomiting had taken place before her admission to the Infirmary, but as it came on a few hours afterwards, I was sent for. I found that she had vomited about six ounces of a coffee-coloured fluid containing blood; her pulse and temperature were rapidly rising, and she was evidently getting much worse.

I accordingly determined to operate at once, and with the assistance of Dr Hodsdon and Dr George Keith I freely opened up the swelling by an incision some ten inches in length. When the sac was opened it was found to contain a large coil of gangrenous transverse colon, several large masses of omentum adherent to the sac and also gangrenous, and fluid fæculent matter which had escaped from the ruptured gut.

There appeared to be no strangulation at all at the umbilical opening, and the gangrene seemed to have been caused by the pressure brought to bear on the contents of the hernia between the firm binder above and the pregnant uterus below.

We proceeded to cut away fifteen inches of colon which were absolutely gangrenous or beyond recovery, cleared out the putrid omentum and most of the sac, and having washed the wound out thoroughly with sublimate lotion, stitched the cut ends of the gut to the edges of the skin wound, and got the patient back to bed in an apparently dying condition.

I will not detain you by a detailed account of the fight between life and death for the few days following the operation; how a condition of profound collapse, as evidenced by a fluttering pulse of 160, Cheyne-Stokes respiration, and delirium, gradually gave way before assiduous treatment. On the second day after the operation the bowels moved through the wound for the first time. On the day following, *i.e.*, 17th April, a seven months' child was born alive with one pain, the whole labour being over in the course of three minutes. The child survived about two days. The bowels were again freely moved during the labour, and there was considerable post-partum hæmorrhage.

The patient seemed to be considerably relieved by getting her labour over, and all went well with her till 20th April, when, after a severe rigor and rise of temperature, she developed an attack of phlegmasia dolens, first in the left leg, and, a few days subsequently, in the right. This, however, ultimately cleared up, and she left the

Hospital on 23rd June with the wound well contracted and healed round the artificial anus. The bowels at this time were acting loosely about six times a day, and she was in very good health.

During the three months that she was out of the Infirmary she was confined a good deal to bed, for there was considerable prolapse of the intestine and discharge of fæces whenever she walked about much. Her discomfort was so great that she expressed a very strong wish to come back and have the artificial anus cured at any price, for, as she expressed it, "she would much rather die than go on as she was."

I had thought of trying to close the artificial anus by some modification of Dupuytren's operation, but was dissuaded from this by finding that fluids injected from the umbilical opening into the lower segment of gut would not pass per anum, but regurgitated alongside of the syringe. This appeared to be due to a kink in the gut at the splenic flexion about eight inches from the cut end, which kink was presumably caused by the dragging of the gut downwards towards the umbilicus. The patient was accordingly prepared for the operation of resection of the gut and suturing of its free ends. For ten days previous to operation she was directed to be fed entirely on peptonized fluids, partly by the mouth and partly by the bowel, and the intestines were still further emptied by castor oil and several enemata both from the umbilical opening and from the anus proper. This last proceeding resulted in the passage of a scybalous mass of large size per anum.

The operation was performed on 30th Sept., and again I had the assistance of Drs Hodsdon and Keith. Having carefully cleansed the skin around the opening and having washed out the two cut ends of intestine, I put a ligature round the upper one to prevent fæces escaping into the wound, and then I freed them both from the adhesions which bound them to the umbilical ring and surrounding parts. Gentle traction was then made upon the two ends until normal gut covered by peritoneum protruded sufficiently for my purpose. Instead of using a clamp I passed a piece of thin india-rubber tubing through a small hole in the mesentery and round the gut, fixing it there with a pair of catch forceps; I then cut away four inches of the upper segment of colon and three inches of the lower, with portions of mesentery attached. The two pieces of intestine were, as usual under such circumstances, found of very unequal size, the upper segment admitting four fingers, while the lower, in which function had been in abeyance for five months, would only just admit the forefinger. Though I succeeded in dilating it a little, this inequality in the two ends added very greatly to the difficulty of the operation. This difficulty has been got over in one or two cases by cutting the lower bowel away obliquely from its mesenteric attachment so as to increase the area to be sutured. By the very careful passage of over a hundred stitches I was able to get the ends satisfactorily together.

I used fine curved needles, round like a dressmaker's needle, and threaded with the finest Chinese twisted silk. The method used was the modification of the Lembert suture by Czerny, *i.e.*, a double row of interrupted sutures, which lie completely outside the bowel, and are passed through the serous and muscular coats of the intestine alone. The outer row was about one-third of an inch outside the inner row, and the stitches of one row were about a line distant from one another.

The cut edges of the mesentery were then sutured together, and the gut was returned into the abdomen. The large umbilical opening was brought together by deep silk stitches, and a firm pad and binder applied. A hypodermic injection of $\frac{1}{4}$th of a grain of morphia was given, and the patient was put back into bed, having been just three hours on the table.

During the first twenty-four hours after the operation there was a little fever and restlessness, the temperature once rising as high as 102°, but in a day or two it became practically normal, and has remained so ever since. Wind passed freely the day after the operation, and the day following there was a free liquid movement of the bowels. From that time she has made an uninterrupted recovery, the bowels moving about every other day. She was fed exclusively on peptonized fluids for three weeks after the operation, but for the last six weeks has taken soup, porridge, minced chicken, etc., and is now taking ordinary diet. There has never been any pain beyond a little occasional griping from the passage of wind along the bowel; but that has now quite ceased. A little compound liquorice powder is taken every two or three days to keep the stools soft.

She was discharged from the Infirmary on 25th November in excellent health.

Without attempting to generalize from so small an experience, there are certain convictions which have forced themselves upon me in connexion with this case.

Firstly, that it is of the utmost importance, even in the case of a surgeon who is frequently operating, that this operation should be practised on the dead body before trying it on the living. The confidence that one has really closed the gut, and the celerity in operating, which are obtained by such practice, cannot be too highly valued, for one cannot test by any satisfactory means on the living body whether the sutures have been applied in such a way as to render the gut water-tight before it is returned into the abdomen; and it is most important not to waste any time in the performance of an operation which is very tedious at the best.

Again, judging from several hours' practice of the operation, I cannot say that I see the necessity for the introduction into the lumen of the gut of any of those materials, such as india-rubber collapsible bags, lumps of cocoa-butter, etc., which have been used for the purpose for dilating the gut during the process of suture. As a

matter of fact, I believe that most surgeons who have had any experience of the operation are now coming to this opinion.

With regard to the clamps which are recommended for this operation, though I had the two most frequently used (Bishop's and Treves') by my side, I used neither of them, as they appeared to hamper one's movements very considerably, and neither of them were long enough in the blade to efficiently clamp the large intestine. The piece of india-rubber tubing, applied as I have described, acted admirably. The best form of clamp appears to be that of Mr Makins of St Thomas's Hospital. It is less complicated and cumbersome than the others, and has the advantage both over the method I used and over Treves' clamp, that no wounding of the mesentery is necessary for its application. This is somewhat important, as the experiments of Rydygier and Madelung go to show that separation of the gut from its mesentery is apt to be followed by gangrene of the bowel at the denuded part.

The idea that an assistant's fingers should take the place of a clamp does not commend itself, for neither would the control of the gut during a long operation be so reliable, nor does this plan leave the operator so much room to work in, unless more healthy intestine is pulled out of the abdomen than would be necessary if a clamp were used.

I was prevented upon the occasion of the first operation, by the extreme prostration of the patient and the intense putridity of the parts concerned, from suturing the gut and returning it into the abdomen at once.

It has been established, chiefly by statistics drawn up by Mr Makins, that the best results after resection of intestine are obtained in cases of operation for the cure of artificial anus (the mortality in this class being 38 per cent. as contrasted with a mortality of about 50 per cent. when all cases are taken together). In other words, it appears less hazardous to do the operation in two stages rather than complete the operation at the time of the removal of a gangrenous portion, when the patient is frequently in an unsatisfactory condition, both locally and generally, for such a serious proceeding. It is, of course, undeniable that the patient has in the former case to undergo two operations instead of one in the latter; but it seems probable that this apparent disadvantage is in some instances, at any rate, more than counterbalanced by the lessening of the mortality after the operation when it is done at the later stage. Upon the other side must be borne in mind the increased difficulty in the secondary operation due to shrinking of the lower disused portion of intestine.

Of the thirty-three different methods of suture which have been recommended for the purpose, the one I employed, namely, Czerny's modification of Lembert's, is most in favour. It certainly appears to fulfil the chief requirement of bringing two broad surfaces of the peritoneal aspect of each segment in

good apposition with one another; and judging from experiment on the dead body, the double row of sutures is evidently of service in preventing the *immediate* escape of fluids from the sutured bowel.

A study of the reported cases of resection and suture of intestine shows that, while a certain number of successful operations have been performed on the small intestine, chiefly by German surgeons (and amongst others by the late Dr Angus Macdonald, who, in the course of an operation for ectopic gestation, removed some six inches of small intestine), the large intestine has very seldom been successfully dealt with in like manner.

Mr Hardie (*Medical Chronicle*, Jan. 1885) reports a case where he resected three inches of colon for artificial anus with a successful result. Mr Kendal Franks has within the last few days published a case in which he successfully removed a large epithelioma with six inches of colon; and Professor Weir of New York informed me a few weeks ago that he had in one case resected six inches of colon with a good result.

In the case I have brought before you, no less than twenty-two inches of large intestine were removed in the course of the two operations.

The President said he believed he was only expressing the feeling of every member present in saying how much the Society was indebted to Mr Cotterill for the communication just read. The case had been a very remarkable one; many great and remarkable operations had, since the founding of the Royal Infirmary, now more than 100 years ago, been performed within its walls; some of them, in one way or other, not having obtained sufficient credit throughout the world as having originated or been brought to perfection in Edinburgh. Not a few of these remarkable operations had been recorded in the minutes of the Medico-Chirurgical Society, and the operation just detailed was one most certainly deserving a distinguished place among them. Laparotomy, in the most extended signification of the term, had of late years claimed for itself a new position in operative procedure, not only in obstetric practice, but in the treatment of various diseases of the stomach, liver, gall-bladder, kidneys, intestines, and in those complications sometimes involved in the different forms of hernia. He should wish to hear the remarks of some of the surgeons present on such a subject, and trusted it would meet with a full discussion towards its further elucidation.

Professor Chiene considered Makins' instrument an improvement on the other clamps which were shown. He was favourably impressed by the fineness of the needles used, and though he could not speak from practical experience, Czerny's suture appeared, from what Mr Cotterill had said, an improvement on Lembert's.

Mr F. M. Caird hailed Mr Cotterill's paper as an interesting

addition to what had been done in intestinal surgery. It was a curious fact that in this School the celebrated Benjamin Bell was one of the first to advocate this operation, and was opposed by the no less celebrated John Bell. One of the first difficulties in operating on the intestinal canal lay in the prevention of an escape of fæcal matter. This difficulty was so great that, in the event of being called on in private practice to treat such a case with limited assistance, it was a matter of great doubt how to proceed. Some time since he was called in the middle of the night to a lady well up in years who suffered from acute intestinal obstruction with fæcal vomiting of some days' duration. She had fatty heart, and had recovered from a hemiplegic attack. Abdominal section was performed, and a gall-stone found impacted in the lower intestine. It could not be driven down, and when pushed up was sent back again by the fæcal movement. It was so hard that it could not be broken up. To cut into the gut without assistance in view of fæcal extravasation was not lightly to be thought of. Mr Cotterill's plan of a rubber clamp passed through a hole in the mesentery could hardly be used in such a case, as its tendency would be to make folds in the bowel, whereas they desired to have it flattened out for convenience in suturing. Under these circumstances the intestine was stitched to the abdominal wall, and after the elapse of some hours the stone was cut out. This was done successfully; no fæcal escape occurred. On the fifth day, however, the patient died of cardiac syncope, without a trace of peritonitis. In other instances they had to deal with a gut already ruptured. In the case of an old woman, practically at death's door from strangulated hernia of more than a week's duration, it was thought right to relieve the constriction. As this was divided the gut accidentally slipped back into the pelvic cavity. The patient coughed, and a gush of fæcal matter poured out. There was nothing for it but to extend the incision, find the gut, and stitch it to the wound, so as to make a fæcal fistula. Warm boric lotion was meanwhile being pumped into the peritoneal cavity, and the fæcal extravasation washed out. The patient rallied but slightly, and at the post-mortem the peritoneal cavity was found perfectly clean. Again, some years ago, he had to deal with a gut which was gangrenous. There was no fæcal extravasation. The ruptured part was closed with Lembert's suture, and the patient did well. When a fæcal fistula was established, unless the patient was obviously losing ground, he would not recommend such extensive operations, which involved opening the peritoneal cavity; besides, fæcal fistulæ had a natural tendency to close. He considered that, generally, it was better to adopt some milder measure which was not attended with risk, such as the plan used by Banks, where a moored rubber drainage-tube is inserted. In a case of fæcal fistula which had ensued from an abscess forming after an exploratory laparotomy, he had himself

employed with considerable success the method so well known in Edinburgh of splitting the fistulous area into its original visceral and cutaneous portions, and then dealing with each separately by suture. Finally, in regard to stitching, it was worth noting that Angus Macdonald had used Lembert's suture, but applied continuously, and this had one great advantage over Czerny's, that in cases where time was of importance it could be applied with greater rapidity.

Dr Shaw M'Laren asked if Mr Cotterill could confirm Halsted's statement that, after piercing the serous and muscular coats, the fibrous layer next the mucous membrane could be distinctly felt against the point of the needle. He also desired to know a little more particularly as to the distance of the sutures. He thought it might be possible to test the impermeability of the sutured gut to the passage of fæces before returning it to the abdomen.

Mr Duncan had on a few occasions the opportunity of performing operations somewhat similar to that narrated by Mr Cotterill, but he regretted to say that none of them had been absolutely successful. In one of them he removed the gangrenous portion of bowel in a strangulated hernia, sutured the gut, and returned it to the abdomen. The patient, after four or five weeks, died of a perfectly incontrollable diarrhœa. This might have occurred equally after the formation of an artificial anus. In another instance he performed laparotomy for acute obstruction, and found a small sarcoma of the ascending colon. This was removed, and the ends of the gut sewn together. The operation was one of extreme difficulty, the abdomen being large and fat, and the depth at which the tumour lay very great. It was not successful; some small extravasation took place, and the patient died of septic peritonitis. Regarding the question what they should do with the bowel in strangulated hernia after removal of a gangrenous portion, he thought that, as shown by Mr Cotterill, the formation of an artificial anus was usually better than the immediate suture and return of the gut. What were they to do with this artificial anus afterwards? He thought Mr Caird was wrong in one respect. The tendency of fæcal fistula to heal was not great in what might be called the second stage. Statistics had shown that, after something like eighty days, there was almost no case of spontaneous cure. If the possibility of spontaneous cure after that date were abandoned, how should they treat the artificial anus? Should they perform such an operation as that done by Mr Cotterill, or make efforts to render the passage of fæces from the upper part of the bowel to the lower more easy? Mr Cotterill indicated specific reasons for adopting the plan he did in this particular case, but did not give them a sufficiency of detail. His position was that the obstruction to the passage of fæces downwards was due to a dragging upon the lower part of the bowel, and to a kink at the splenic flexure. This appeared hypothetical, and he did not give sufficient reason for the

performance of the operation when he was not certain that the lumen of the bowel would be open. As it turned out, he was probably correct, and may have had some stronger reason than he gave them for believing that this downward dragging was the cause of the obstruction. Mr Duncan did not, however, think that this operation was the correct mode of treatment for artificial anus in general. When there was a distinct eperon, the first thing was to obliterate it. The septum should be repressed, whether by Dupuytren's method or by the twisting with a wire. In cases where they got the passage made all right, but the fistula refused to heal, they might adopt a method he had recommended twenty years ago, and performed on two occasions with complete success. This consisted in splitting up the bowel, and sewing mucous membrane inwards and skin outwards. It was applicable and most successful in ruptured perineum and other plastic operations; and, at Mr Chiene's suggestion, he had successfully used the method in recto-urethral fistula and harelip.

Mr Miller thought the success of Mr Cotterill's operation was due very much to the fact that it was done *à deux temps*. The fæcal fistula was first formed, and after preparing himself and the patient he carried out what had been a very successful proceeding for its closure. Sometimes it was necessary to perform these two in one operation. In these circumstances time was a most important element. Senn of Milwaukee recently recommended that in such cases the opening in the bowel might be occluded by a flap of omentum and putting in a few sutures on the Lembert principle. He found in experiments on animals that this was perfectly successful in preventing leakage into the peritoneal cavity, and could be very quickly done.

Dr Cotterill, replying, said he preferred clamping the gut with a metal or rubber clamp to compression with the fingers, partly because the latter were in the way, partly because in a long operation they could not be trusted, and partly because their heat seemed to have an injurious effect on the bowel. He thought it was possible to tell when the needle had passed through the serous and muscular coats. These slid readily upon the mucous coat, which he did not think he had pierced once with the needle in his experiments. He might best give Dr M'Laren an idea of the distance of the sutures from one another by informing him that the sutures in any one row were about a line apart from each other, and the distance between the outer and inner rows was about one-third of an inch. The reason why he had adopted the operation he did in this case was because of the position of the portions of the bowel presenting. They lay parallel to one another outside the abdomen, separated by a mass of fat and cicatricial tissue. There was no sort of attempt at passage of fæces from one to the other. Water injected into the lower portion flowed back along the sides of the tube. A rectal bougie could not be passed towards the

rectum. It was not till he got a soft œsophageal bougie past the "kink" that fluids passed per rectum, but after that obstruction was overcome large quantities flowed easily. He considered the septum was too thick to be attacked by any of the ordinary methods for repression. He was sure that these operations by destroying the spur were not so safe as was sometimes represented. He could not agree with Senn's recommendation to put in a few sutures, and the suggestion to test the imperviousness of the sutured bowel by allowing the passage of fæcal matter along it could not be carried out with safety; and, moreover, one of the things done in preparation of the patient was to get the gut thoroughly washed out. If this was not done, and fæces allowed to escape during the operation, it was a very difficult matter to prevent contamination of the peritoneal surface. Mr Cotterill then alluded to the immunity from risks of peritonitis and septic absorption which were constantly observed in patients who had for some length of time suffered from old hernia, tumour, or other abnormal condition of the abdomen. He concluded by thanking the Society for the extremely kind way in which they had received his paper.

Meeting III.—December 19, 1888.

Professor CHIENE, *Vice-President, in the Chair.*

I. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Mr A. G. Miller* showed—(*a.*) A FRACTURE OF THE LOWER EPIPHYSIS OF THE FEMUR produced by torsion. The patient was a boy suffering from hip-joint disease for which amputation was considered necessary. The fracture occurred during the operation while the limb in an extended position was being rotated outwards. The case was of interest to him because it was the third time this accident had happened in his hands. The first was when he was a student, rotating a limb for Prof. Spence while he was excising the head of the femur, and the second when he himself was amputating at the hip, as in this third case. A French surgeon, M. Féré, recently described the case of an old woman with a helicoidal fracture produced by rotation, but while the limb was bent. Similar fractures were produced on the dead body in the same way by rotation of the bent limb. In Mr Miller's cases the fractures were all transverse. (*b.*) Along with Dr Alexander Bruce a DISTENDED GALL-BLADDER OBSTRUCTED BY MALIGNANT TUMOUR OF THE DUODENUM. This specimen was from the body of a woman aged 52. She first showed symptoms two years ago, when she suffered from vomiting and constipation with intermittent attacks of diarrhœa, and hæmor-

rhage from the bowel. The enlargement of the gall-bladder was apparent ten months before death, and jaundice three months. She had great thirst and craving for food, but distinctly and frequently denied having pain. It was thought before death that the obstruction was probably caused by malignant disease of the pancreas, but at the autopsy this was found to be healthy, the duodenum being affected. The case was one of great rarity. Murchison in his work referred to only three cases in which a similar condition was found, but in them the pancreas was involved, and Mr W. H. Kesteven had recently recorded in the *Lancet* a case of cancer of the pancreas with biliary obstruction. A cast by Mr Cathcart showing the condition of the parts when fresh was also shown.

Professor Chiene considered the fracture shown was not through the epiphysis, but through the shaft of the femur. It was at least an inch above the epiphysial line.

2. *Dr Allan Sym* showed a TUMOUR OF THE BLADDER from the body of a woman aged 73. It had been operated on by scraping twice. Dr Angus Macdonald had operated first some years ago, and Dr Croom only last August. Both operations were followed by great hæmorrhage. A month after the last operation, for persistent hæmorrhage several styptics were tried unavailingly. The only one that was partially successful was a decoction of the leaves of hamamelis. The greater part of the posterior wall and the fundus of the bladder were involved, but not the ureters nor the urethra. The tumour was a rapidly growing carcinoma.

Professor Chiene said this was the third case in which he had heard of death following the scraping of villous tumours of the bladder. He thought some means might be adopted of draining the bladder after the operation, and preventing the accumulation of urine and the consequent systole and diastole of the bladder, which did not allow of the necessary rest to the part after operation.

3. *Dr James Ritchie* showed a CYSTIC KIDNEY—the left one—which after injection with gelatine and carmine weighed 41 oz. (the right one 27 oz.) The specimen was from a woman, aged 45. Tumour was noticed after a confinement nine years ago, but in the absence of symptoms its nature was not diagnosed. When symptoms became prominent the diagnosis was easy: polyuria, thirst, loss of flesh, and the presence of a large nodular tumour in each flank. The feeling of fluid in one of the larger cysts was easily felt through the abdominal wall. The patient died of uræmia without convulsions, and retained consciousness to the end.

II. Original Communications.

1. *Dr Craig* read a paper on

THE TREATMENT OF DIPHTHERIA IN CHILDREN BY STEAM MEDICATED WITH SULPHUROUS ACID.

By T. Wyld Pairman, L.R.C.P. & S. Ed., Te Awamutu, Auckland, N.Z.

A very material hindrance to the successful treatment of diphtheria in children is the difficulty one experiences in painting the throat with the requisite antiseptic. The operation is a delicate one for the experienced practitioner, and doubly so for those unaccustomed to the handling of children under disease. Indeed, it becomes a matter of doubt whether in some cases the throat had better remain unpainted rather than frighten the children to the verge of convulsions. Again, when the management is left to parents and nurses, the painting is usually done in a very perfunctory and unsatisfactory manner; consequently, the proper action of the medicament is lost. What is wanted is a method whereby the child may lie undisturbed during the process of treatment, so as to obtain the full benefit of a very important factor—*rest*. It is my firm belief that the want of general and local rest is one main cause for the great mortality attending this disease. One can scarcely be surprised at the extreme debility which ordinarily accompanies diphtheria when he remembers that the child is awakened every two or three hours for the purposes of treatment. Should the larynx become affected, the difficulties attending the management are very much increased. Atomisers are useful in such cases, but in epidemics the demand far exceeds the supply, and I have never found them very efficient with children.

With your permission, I shall give you my experience of a method—applicable in all forms of the disease—which has proved extremely successful in my hands during a severe and recent epidemic. Let me first refer to some cases treated in the ordinary fashion.

Case I.—I was called to the first case on the morning of the 27th June 1888. The patient, a girl aged 6 years, resident in the township, had been complaining for some days previously of a sore throat. She was now in a high fever, and on examination the tonsils and pharynx were seen literally covered with a dirty-white leathery membrane. I pronounced it diphtheria, and gave the following injunctions, which I then considered judicious treatment:—Linseed meal poultice to throat; tinct. ferri perchloridi in full doses; chlorate of potash and hydrochloric acid gargle, to be used every two hours; inhalation of steam from a jug of hot water as often as convenient; beef-tea, etc., etc., frequently; and sulphur fumigations as a disinfectant. In the afternoon I substituted a

paste of iodoform and glycerine for the gargle, a preparation I had formerly seen act as a charm. During the night this treatment was adhered to, and the patient appeared to her parents better. But about six o'clock on the following morning they observed the child had a difficulty in breathing, and I was summoned at eight. The disease had spread to the larynx, and asphyxia was imminent. Tracheotomy appeared the only indication, so I performed the operation, with immediate relief to the urgent symptoms. A steam chamber was improvised, and I attended personally to the patient, but she expired from asthenia three hours after the operation.

Case II.—A younger member of the same family was attacked in a similar way on the 29th June. A number of small patches were scattered over the surface of both tonsils, and these ultimately coalesced into one mass, and implicated the entire throat. I treated him with tinct. fer. perchlor. inwardly, and various antiseptics locally. He strongly resisted all efforts to paint his throat methodically, and it was often done in a very indifferent fashion. He remained under my care for a month, and suffered from frequent dysenteric attacks, which were extremely weakening. Ultimately he recovered completely, with the exception of a paralysis of one side of the face and partial blindness of one eye.

Case III.—A girl, aged 5 years, a delicate child from birth, and not long convalescent from hooping-cough. Saw her on 8th July, and discovered the fungoid growth on the left tonsil only; apparently a mild case, but I treated her like the others. She appeared much better on the 9th, but during the night of the 10th a relapse occurred, and on the morning of the 11th I found the disease had spread to the windpipe. Emetics of ipecacuanha were administered without relief. The fumes from burning tar and turpentine—a favourite American remedy—were unavailing, and the child died in a few hours.

Case IV.—A strong, healthy girl of 3 years was attacked on the 8th July with symptoms of laryngitis, and treated by her mother with vin. ipecac. and tinct. aconiti. She did not improve, and I was called in on the 10th only to find her gasping for breath, and the whole throat a mass of diphtheritic slough. I swabbed out the throat with a solution of permanganate of potash, made her inhale steam constantly from a jug of hot water, and otherwise treated her in the usual manner. The steam appeared to give great relief, but she ultimately died from asphyxia—the parents refusing to allow tracheotomy.

I was now thoroughly convinced that in this epidemic, at any rate, the usual orthodox treatment was inefficient, and as I had already observed the relief afforded by the inhalation of steam, I determined to give it a more decided trial.

Case V. soon presented itself. A boy, 5 years old, showed the usual diphtheritic symptoms, and very soon the larynx gave evidence of being affected. I followed out Oertel's plan of inhaling steam for fifteen minutes twice an hour, and attended also to general and local treatment. After a few hours, the throat certainly showed signs of improvement, but the laryngeal symptoms remained as before, and about midnight I was hurriedly called to see the child. I found him in the greatest distress, cyanotic, and in immediate danger of suffocation. I erected a tent over the child, and led a pipe from a boiling kettle to the bed. He was soon in an atmosphere of steam, and in a few hours his colour improved and he became less uneasy. At the end of thirty-eight hours he coughed up a cast of the trachea, and from this time his recovery was assured.

It now became apparent to my mind that in constant steam inhalation we possessed a very powerful remedy, and after treating over 25 cases by this method without losing a single patient, I have deemed the subject sufficiently important to bring before the notice of this Society. I have given you the history of five cases to enable you to judge the nature of the disease, and to show you the inutility of the usual remedies. You will observe that the fungoid growth in four of these cases extended to or began in the larynx. This feature of the epidemic was peculiarly striking, more than half of the cases having shown laryngeal symptoms.

Practitioners who have long practised in this colony tell me they have often remarked that in epidemics the fungus appears to choose a particular site for its nidus, sometimes attacking only the pharynx, and at others extending to the windpipe. In this semi-tropical climate, also, the disease has a very active growth, and except prompt measures are taken to arrest it, the effects are very deadly. I have known a clean tonsil covered with membrane in the short space of three hours.

My invariable practice now, when called to a case of diphtheria, is to erect a tent over the head of the sufferer by fixing to the bed an open umbrella, and throwing over this a large sheet. I then procure 6 feet or so of zinc spouting, which can be readily detached from the dwelling-house, punch a hole in its length, and fit the mouth of an ordinary kettle into it. One end is tilted up and placed under the tent, the bed being situated at right angles to the fender, and if the kettle contain water at no higher level than the inner spout-hole, steam will at once proceed up the pipe into the tent. With this steam treatment I usually combine mild antiseptic fumes, obtained by burning sulphur in the apartment. Nothing more is required. Should the patient be an adult, I may order the throat to be painted with an antiseptic three or four times a day, but this is not at all necessary. I have successfully treated many simply with the steam and sulphur fumes, and some of the cases have been of a very virulent type. As a prophylactic

during convalescence, I advise the use of some chlorate of potash preparation. Let me refer to one other case as illustrative of my remarks.

CASE VI.—At 3 P.M. on the 3rd September 1888, I was asked to visit a child, 5 years old, who had been ill for some days. Her father stated that she was in a burning fever, delirious at times, unable to swallow, and had a difficulty in breathing. I visited her at once, and recognised the nature of the case. Both tonsils were covered with the fungus, and, judging from the respiration, the larynx was only slightly affected. I rigged up the tent and apparatus already described, and left my injunctions with a trained nurse. These were, to keep the steam going constantly, to burn a teaspoonful of sulphur every hour, and to let the patient have as much milk as she could drink, as well as beef-tea, chicken broth, *et hoc genus omne*. At seven o'clock she fell into an uneasy slumber, and awoke at 10 much better, partook of a good draught of milk, and said she liked the steam. At midnight she fell into a deep, natural, and soothing sleep, and awoke at seven on the following morning with all the bad symptoms gone, and an intense craving for food. I saw her at three o'clock in the afternoon, exactly twenty-four hours after my first visit, and she appeared perfectly well. I examined the throat; the fungus had gone, and the only sign to show there was anything wrong was an enlargement of the tonsils, and an excavation in each showing where the enemy had been rooted out. The nurse informed me that the fever began to abate about three hours after commencing the treatment, and had entirely left by midnight. I then stopped the steam supply, ordered the room to be kept warm, and gave the patient a few compressed tablets of chlorate of potash to suck slowly for some days.

This case is a fair type of many that I have treated in a similar manner, with equally gratifying and remarkable results.

I will conclude by enumerating some of the advantages of this method of treatment.

1. Constitutional and local rest are obtained during the whole course of the disease.

2. The steam acts as a very efficient poultice. We may consider the fungoid growth essentially as a slough which requires to be removed, though not forcibly torn away. To assist its removal heat and moisture are applied, as we would apply a poultice to any slough elsewhere. Furthermore, the antiseptic reaches the active micrococci, which are most abundant in the underlying tissues.

3. The method is applicable in all forms of the disease. Even in cases where the air-passages were very much involved, I have seen great relief in a few hours, but usually some days are necessary to effect a cure.

4. Very little apparatus is required, and the method is extremely

simple. This is an important point in country practice, especially in the colonies, where one is often driven to extremities for suitable appliances. Oertel's plan is not, in my opinion, sufficient. It is like allowing a poultice to become cold, which then may do more harm than good.

5. Paralysis seems never to occur as a sequela.

6. Patients very seldom complain of any sore throat, and are able to swallow food with little or no inconvenience.

In conclusion, the method may appear an extremely weakening process, but I have never found it so. Convalescence is very rapid; and in my experience, if proper precautions are taken against cold, no bad effects have followed, even in cases where children have breathed steam four days and nights successively.

Professor Chiene expressed his sense of the value of Dr Pairman's paper. He did not quite understand if the child was completely tented in.

Dr Craig said he believed Dr Pairman's usual plan was to place a large umbrella opened out at the head of the child's bed, and to put a sheet over this extending to the foot.

Prof. Simpson, in the course of a few remarks, pointed out that the title of the paper was somewhat misleading. It was not antiseptic steam, that was to say steam containing some volatile antiseptic, that was used, but ordinary steam and sulphur fumes. In the first case he understood it was steam alone that was used, and this did well. The sulphur fumes were introduced into the treatment of the other cases. He would have been glad had there been a little more careful analysis of the various elements in the treatment.

Dr P. A. Young was interested to hear of Dr Pairman's success in the treatment of cases of diphtheria by steam. He had himself treated cases in this way for at least twelve years. He had been using it generally for some time, but he had a very bad case in a young child, and found that it received the greatest benefit by being kept in an atmosphere of steam for four or five days, after which it recovered. He was satisfied that they possessed no remedy so efficacious. Superadded to it he often used antiseptics. Some time after having this case he read an article by a French physician, who said the action of the steam was to produce a layer of healthy pus cells between the false and the mucous membranes. In this way the false was thrown off and further absorption into the system prevented. He did not know how far this was a correct explanation of the *modus operandi*, but he gave it for what it was worth. Hooping-cough, where there was bronchial catarrh to any extent, might be efficaciously treated in the same way, various antiseptics being added to the steam.

Dr M'Bride was of opinion that steaming with antiseptics was not likely to be of much more value than simple steam, itself an

old-established and efficient remedy. Strong antiseptics applied in the form of sprays and pigments constantly failed in the treatment of diphtheria. It was hardly to be expected that a dilute volatile antiseptic would succeed in such cases.

Dr Thom (Crieff) said he could not trust to volatile antiseptics. He found steam very valuable if applied before the disease extended to the larynx. When this occurred he had been uniformly unsuccessful. He frequently used as a pigment a strong antiseptic containing perchloride of iron and sulphurous acid. He found a pigment of boroglyceride in glycerine very beneficial and more pleasant to children. Sulphur was a very old remedy, and flowers of sulphur were constantly used in his district.

Mr Catheart observed that in the discussion there was no confirmation of a statement he had read some time ago, that steaming, particularly over-steaming, did harm. He was of opinion that weak volatile antiseptics constantly applied might prove efficacious where the stronger antiseptic applied intermittently with a brush failed. While much was to be said for steaming, it was not to be supposed, as one might imagine from what had been said, that the patient only required to be steamed to be made well. Constitutional treatment was also necessary, but many cases proved fatal in spite of all treatment. One remark made in the paper that in different epidemics the fungus seemed to choose different sites was, he thought, an argument in favour of the non-identity of croup and diphtheria.

Dr Webster desired to add his testimony to the value of steaming in these cases. He was somewhat disappointed with the paper, hoping from the title that they were to hear of some new and successful antiseptics from the Antipodes.

Dr Allan Sym said he had lately to deal with an epidemic in his district. He treated his cases by pigments of boroglyceride, and also gave perchloride of iron internally. Having, however, taken the disease from a patient, he experienced so much pain in swallowing that he had given up internal remedies, and simply painted the throat with the boroglyceride, being quite as successful without internal treatment as with it.

Dr Maxwell Ross said that in more southerly parts of New Zealand, particularly round about Dunedin, the treatment of diphtheria by blue gum steam was exciting some interest, and some very successful results were claimed for it. He believed that a gentleman had gone from Dunedin to some of the larger Australian towns to demonstrate its value, but he had been disappointed in the leaves of the blue gum grown in Australia, which he did not find give such good results. Dr Sym, he understood, had given up internal administration of remedies because of the pain in swallowing. He did not think pain was a symptom of diphtheria, and if it occurred, considered it was due to some accompanying inflammation such as tonsillitis. He thought it

would be a mistake to give up internal treatment, and seeing that many of these cases died of cardiac failure, he asked if any member had experience of digitalis in diphtheria.

Dr James Ritchie said he wished Dr Pairman had given his experience of the relative merits of steam and of the antiseptic. The speaker used steam in croup, antiseptics in diphtheria. But in children the difficulties connected with the use of pigments and sprays were so great that some efficient method to replace these was very desirable. Dr Pairman had recorded certain facts and made certain deductions which might be criticised, but Dr Ritchie would make trial of antiseptic steam. The speaker never used strong caustics in this disease. He found pain to be a very variable symptom, and he believed it to be due to inflammation, apart from the diphtheritic process, at least not a necessary part of it. He believed that the disease was primarily local and secondarily constitutional from poisoning of the system by ptomaines. Death from cardiac syncope was only one of the forms of post-diphtheritic paralysis.

Mr Miller asked Dr Sym if he had any experience of steaming, and if he had found it of any help in his own case?

Dr Sym replied that it was not used.

Mr Miller, continuing, said he had long wished to have a method of treatment devised which would prevent the necessity for tracheotomy. No treatment had hitherto proved satisfactory in that respect, and when discussing modes of treatment which appeared to be attended with success, they must remember that in epidemics of any kind they might have a series of fatal cases followed by a long run of successful ones—recovering, apparently, whatever mode of treatment was adopted. He considered more attention should be paid to the prophylaxis of diphtheria than was done at present. An epidemic of diphtheria should be looked upon as a disgrace to medicine and to civilisation.

Dr Craig was very pleased that the paper he had read for Dr Pairman had elicited such an interesting discussion. Dr Pairman's treatment appeared to have been very successful. They had a very volatile antiseptic in sulphurous acid, which he sent into the atmosphere by burning a teaspoonful of sulphur every hour. His father, the late Dr Pairman of Biggar, long ago practised this mode of treatment. The steaming had been successful in some of the cases in which the disease had spread into the larynx, and certainly no harm could be said to have been done by it to any of his patients. Dr Ross had referred to the difference found in the eucalyptus leaves in New Zealand and Australia. This was not an uncommon experience. The late Sir Robert Christison had pointed out that certain umbelliferous plants were found to be poisonous when grown in one district of country, but quite harmless when grown in another. He should ask Dr Pairman to inquire into the point that had been raised,—the effect of

steam alone, and steam with sulphur fumes. He would, however, like to point out that there might be a difference in the disease due to the semi-tropical climate from what they usually found in this country.

2. CLINICAL NOTES ON FATTY TUMOURS OF THE LARYNX.

By P. M'Bride, M.D., F.R.C.P. Ed., F.R.S.E., Surgeon Ear and Throat Department, Royal Infirmary, and Lecturer on Diseases of the Ear and Throat, Edinburgh Medical School.

My reasons for calling attention to lipomata of the larynx are—(1), that these growths are of extreme rarity; and (2), that within the last eighteen months I have operated upon two cases of this kind.

From a perusal of the most recent literature on this matter, including the second edition of Gottstein's *Krankheiten des Kehlkopfes*, published this year, I gather that, so far, only in two cases have fatty tumours been removed from the larynx *intra vitam*, and that the operators have been Bruns and Schrötter. In two other instances Wagner and Tobold noted the presence of lipomata on the dissection table.

At this juncture I should like to turn aside for a moment to point out certain discrepancies which I have discovered in my literary researches concerning a subject so limited that one might at all events expect, from those who consider themselves authorities on laryngeal diseases, an approach towards approximate accuracy. Eppinger (*Pathologische Anatomie des Larynx und der Trachea*, p. 205) quite correctly mentions the three examples which had been observed up to the year 1880,—to wit, those of Wagner, Tobold, and Bruns. With regard to Wagner's case, he gives the reference as *Archiv für Prakt. Heilkunde*, 1872, p. 108, and states, in a somewhat aggrieved tone, that the growth was as large as a hen's egg, and that there is no further account of "its attachment and relation to the interior of the larynx." Now I have, shall I say, verified this reference, and found it in the corresponding part, not of the *Archiv f. prakt. Heilkunde*, but in the *Archiv f. Heilkunde* (1872, p. 108), and I have further found that the lipoma referred to was attached "by a thin broad base to the middle line of the tongue and anterior part of the epiglottis." This growth, I may add, was found in the body of a girl of thirteen who had died of small-pox, and Wagner further mentions that she was to have been operated upon by Wendt. From this we may, I presume, infer that no immediately urgent symptoms existed. Eppinger also mentions Tobold's case as being described in the *Archiv f. Prakt. Heilkunde*, 1872, p. 422. I do not know whether such a journal exists, but the *Archiv f. Heilkunde* (in which the preceding case was found) does not, at the page named,

contain any reference to a lipoma of the larynx. So much for Eppinger's accuracy. But if we now turn to Schrötter's paper ("Ueber das Vorkommen von Fettgewehe im Larynx," *Monatsschrift für Ohrenheilkunde*, June 1884), we find exactly the same inaccuracies repeated. In Gottstein's work the erroneous references are not reproduced, possibly because no exact references are found in the book, but it is expressly stated that in Wagner's case the point of attachment is not given, showing that no attempt can have been made to verify his statements (*Die Krankheiten des Kehlkopfes*, 1888, p. 149). Now, as I cannot find Tobold's case described by any other authors than those referred to, it is not surprising that I know little about it, and that also, for obvious reasons, I refrain from reproducing what little is mentioned by these authorities. I have no desire to act the part of a captious critic, but the evil illustrated by my investigations into a subject so limited as lipoma of the larynx is, I believe, widespread. Authors are too apt to follow each other like sheep without verifying data, and thus, instead of perpetuated truth, we have perpetuated error. To grasp the whole of medical science is impossible for one man, but, on the other hand, it is not too much to ask of those who would be our instructors and pioneers that they should first instruct themselves.

To return to our subject, then, we find that the authentic cases of laryngeal lipoma, so far as can be ascertained, are three in number. That recorded by Wagner has already been described. In Bruns'[1] patient, who was twenty-five years of age, the tumour grew from the left half of the posterior laryngeal wall, filled the whole glottic aperture, and was removed by repeated use of the electric cautery. Schrötter's case (*op. cit.*) was in several respects peculiar.

The patient, a man of 55, two years before his examination, began to suffer from a tickling cough. Fifteen months later he began to have the feeling of a foreign body in the throat, and eventually distinct dyspnœa with nocturnal exacerbations became marked. Until a few weeks before examination the voice had been quite good, and even afterwards it was only intermittently interfered with. These attacks of aphonia, however, were relieved by coughing, and on such occasions the tumour was driven up into the pharynx. Deglutition was never affected.

On laryngoscopic examination, the whole of the larynx, except the right margin of the epiglottis and the right ary-epiglottic fold, was covered by a tumour. The appearances, however, varied from time to time, and after great difficulty it was found that there was a growth attached to the left margin of the epiglottis, the corresponding pharyngo-epiglottic fold, the lateral wall of the epiglottis, and the left ary-epiglottic fold by a broad pedicle. The tumour was divided into finger-like processes, and thus the variations in

[1] The original was not accessible.

the laryngoscopic appearances were accounted for. The tumour was eventually removed in several sittings by means of the galvano-caustic snare, and proved to be a lipoma, as was at first conjectured on account of its light colour, lobulated appearance, and soft consistence.

I shall now briefly recount my own cases.

(1.) M. W. was brought to me by Dr Carmichael on the 23rd July 1887. The history of the case was as follows:—About six months ago the patient felt that occasionally in breathing he made a peculiar noise, but this occurred only for two or three minutes once or twice a day. Eventually this symptom became troublesome when the patient lay down, and finally he began to experience difficulty in swallowing. There was no pain, but deglutition required a distinct effort, and was accompanied by a sound audible to those in the neighbourhood.

On examining the throat a pale pink rounded tumour was seen behind the tongue. Examination with the laryngoscope and probe showed that it was attached to the epiglottis, and about the size of a pigeon's egg. As my electric cautery was not ready for use at the time of the patient's first visit the wire of Jarvis's snare with a bent shaft was passed over the tumour and tightened. To make a long story short, it was found that the wire could not be made to penetrate the growth, and the part seized had eventually to be cut off with bent scissors. This portion comprised about a half of the tumour. The remainder was easily removed by means of the galvano-caustic snare adjusted by the aid of the laryngoscope. The first portion of the growth removed was handed over to my friend Dr Alex. Bruce, who described it as a fibro-lipoma. The other half I now show you. The stump which was left had a broad attachment to the right valleculæ and adjacent part of the dorsum linguæ. At first the sloughing resulting from the electric cautery produced fœtor of the breath and a bad taste in the mouth. These symptoms soon yielded to the use of a boracic acid gargle. On the 26th December 1887 the condition of parts was as follows: "Only a rounded fulness filling up right vallecula and evidently attached to the epiglottis and adjacent part of the tongue." On the 3rd December 1888 Dr Carmichael again brought the patient to me, and the tumour had not only recurred, but reached a size equal to that when it was first examined. To this recurrence I attach little importance from the point of view of ultimate prognosis. At the same time, it is evident that a tendency to reappearance will exist so long as any part of the tumour is left, and it will be no easy task, situated as it is, to extirpate the growth together with its capsule by endo-laryngeal operation.

My second case of laryngeal lipoma is but of recent date.

P. B., æt. 71, porter, applied for treatment at the Royal Infirmary on the 16th November 1888, giving the following history:—Some five or six months ago the patient felt a lump in his throat,

and though aware of its presence did not find it obstruct the breathing, cause pain, or interfere with swallowing. It gradually enlarged, causing, last September, a thickness of speech and obstructing the breathing when the patient lay on his left side. Latterly difficulty in swallowing has been experienced, but there has been no pain at any time. Sleep has never been disturbed.

On laryngoscopic examination, a pale pink mass was seen overlying the left arytenoid cartilage. On touching this with a probe a cough resulted, which shot the tumour up to a level with the dorsum of the tongue. The growth was eventually seized with a vulsellum; the snare of the electric cautery was then passed over it and pressed well down, tightened, and the tumour removed. The latter was fully as large as a bantam's egg. A fragment removed was examined by Dr Martin and found to consist of fatty tissue. I now show you the lipoma in question, and it will be at once noticed that it has also a distinct capsule. After removal it was seen that the pedicle was narrow, and had been attached to the outer part of the right pyriform sinus.[1] The patient was kindly given a bed for a day or two by Professor Annandale, because I feared that laryngitis might possibly result from the effects of the electric cautery. No such untoward result, however, supervened, and a week after the operation only a white eschar was visible on the outer and anterior part of the sinus pyriformis. From the very few cases so far published it is impossible to generalize as to the diagnosis and prognosis of fatty tumours of the larynx. We may, however, say that they are characterized—(1), by their light pink colour; (2), the disproportion between their size and the symptoms they produce; and from my first case we are also entitled to conclude that unless the removal be complete rapid recurrence may take place.

Since writing the above I have again operated on my first case, in which, as will be remembered, recurrence took place. The tumour was seized with a strong and heavy pair of catch forceps. Over these the galvano-caustic snare was slipped, and the growth which I now show you removed. As will be observed, it has the appearance of having been enucleated rather than cut off. On examining with the laryngoscope after the operation, from which virtually no bleeding resulted, there was seen to be an excavation corresponding to the point of attachment. It was further noted that the right margin of the epiglottis was somewhat excavated, a condition probably due to the pressure of the somewhat pendulous tumour, which, although attached by a broad base, tended to hang over into the laryngeal cavity.

This last operation seems to indicate the advisability of seizing not only the capsule of the fatty tumour, but also the adipose

[1] From a strictly anatomical point of view it may be contended that this was not "a fatty tumour of the larynx." If it be preferred to call it "a fatty tumour in the larynx," this in no way detracts from the interest of the case.

tissue, so that the latter may be drawn from its bed and enucleated as completely as possible. I may mention that when the patient was last examined cicatrization was not completed, but that no reaction of any note had followed the removal of the growth.

Mr Cathcart asked if the term laryngeal was strictly applicable to these tumours. It seemed to him that the first at any rate grew from the back of the tongue rather than the larynx. Tumours of the size shown growing into the larynx must have produced severe obstruction to respiration.

Dr M'Bride said he, in the first place, pleaded precedent for calling these tumours laryngeal, and in the second, two of them grew from laryngeal structures, while the third, although growing from the sinus pyriformis, projected into the larynx and covered one arytenoid cartilage. Though so large they had a wonderful power of adapting themselves to the sinuosities of the larynx, so much so that they might at times be missed even by those accustomed to make laryngoscopic examinations.

Meeting IV.—January 16, 1889.

Professor SIMPSON *in the Chair.*

I. NEW MEMBERS.

The following gentlemen were elected Ordinary Members of the Society:—Charles H. Bedford, M.B., M.R.C.S., F.S.A., 2 Windsor Street, Edinburgh; William Haldane, M.D., Viewforth, Bridge of Allan; J. H. A. Laing, M.B., C.M., 11 Melville Street, Edinburgh; Harold J. Styles, M.B., University.

II. PATHOLOGICAL SPECIMENS.

1. *Dr Alex. Bruce* showed—(*a*), two specimens of MELANOTIC SARCOMA OF THE LIVER occurring after removal of the eye-ball for melanotic disease; and (*b*), a CARCINOMA OF THE LIVER spreading along the portal vein.

2. *Mr F. M. Caird* showed a specimen in which there was an ARTIFICIAL ANUS on one side with a return of HERNIA on the other. The patient was admitted with a femoral hernia which had been down for some time. During the operation the gut gave way. It had to be brought to the surface and an artificial anus formed. The peritoneal cavity was washed out with warm boracic lotion. Death occurred in eight hours. At the post-mortem examination it was noticeable that there was no fæcal matter in the abdomen.

On the opposite side there was a cicatrix, the result of an operation for hernia by Dr Cotterill six years before. A new hernial sac had formed and gone down the old course.

3. *Dr Francis Troup* showed two specimens of THORACIC ANEURISM. They were chiefly remarkable for their large size. One of the patients obtained great relief from the use of ice, and it was remarkable that, though his skin was kept at a low temperature for a very long time, no sloughing took place, and he ultimately died of exhaustion.

4. *Dr Philip* showed MEMBRANOUS FRAGMENTS passed per urethram by the patient whose case he read afterwards.

III. THERAPEUTIC AGENT.

Dr Philip showed an emulsion containing cod-liver oil to the extent of 75 per cent., and eucalyptus oil in the proportion of twenty minims to the tablespoonful. For about two years he had used the pure oil of eucalyptus internally with great advantage in a large number of cases of phthisis pulmonum. The emulsion was the result of many attempts made, at his suggestion, by Messrs Baildon & Son, for the purpose of combining the drug with cod-liver oil. It had the further advantage that the eucalyptus oil disguised the taste of the cod-liver oil better than any other agent he knew.

IV. ORIGINAL COMMUNICATIONS.

1. NOTE ON A CASE OF ELEPHANTIASIS ARABUM.

By R. W. FELKIN, M.D., etc., Lecturer on Diseases of the Tropics and Climatology, Edinburgh School of Medicine.

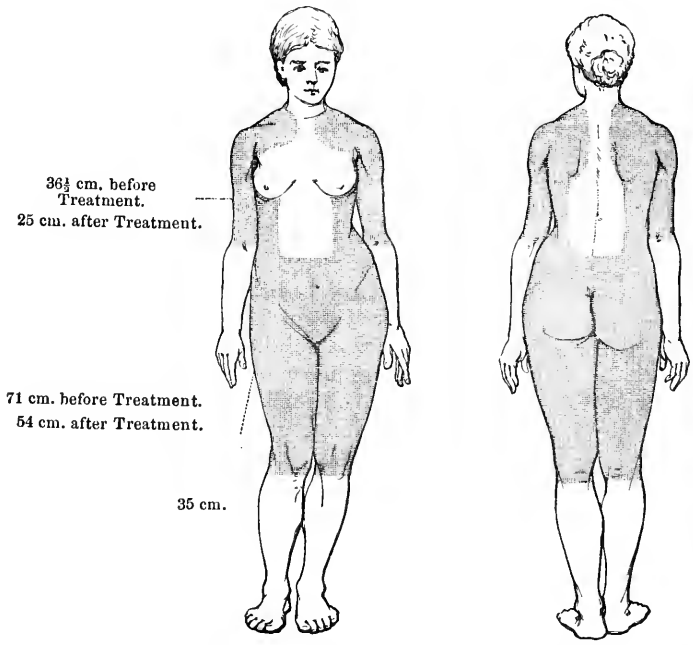
I AM induced to bring this case under the notice of the Fellows of this Society for three reasons—firstly, because elephantiasis is rarely seen in this country; secondly, because the case presented various peculiarities; and, lastly, because the treatment resulted in an, at any rate, apparent cure.

On the 14th of July 1888 I was asked to see an unmarried lady, a Eurasian. Her age was 33, her height 5 feet 2 inches, and her weight 11 stones 4½ lbs. She was born in India, and had resided there most of her life. It was only with considerable difficulty that I could obtain any details from the patient as to her previous history or the commencement of her ailment, so that I am not able to give many particulars which would have made the case more complete. She had never seen a doctor professionally in her life before, and greatly objected to doctors, but I managed to ascertain that when a child she had not been able to walk properly. She could give no reason for this, but she explained her horror of

doctors by the fact, that one of her earliest recollections was that of a lady constantly threatening to take her to see one if she did not try to walk better. She said she had always enjoyed good health, with the exception of suffering now and then from intermittent fever. She had led an active life, and had apparently undergone considerable exertion. Up to the beginning of 1887 her appetite had always been good, but she had been remarkably thin. After a rather severe attack of fever early in 1887 she noticed that she commenced to increase in size, and that her appetite was capricious. She took a great dislike to fish, and always vomited after eating it. She had also occasional attacks of nausea and vomiting with headaches. Every now and then her occupation became rather tedious to her, and she sometimes felt as if she could hardly keep up. In September 1887, as she felt gradually growing worse, she decided to leave India for a time; and for some unknown reason she was weighed before starting on her journey, and felt surprised to find that she weighed 9 stones 8 lbs. The voyage home seems to have done her good, for when she landed in England she felt better than she had done for some time previously, and this improvement in her feelings continued for two or three months. She noticed, however, that she still increased in size, and that her skin was at times itchy and very irritable. Her limbs, too, felt painful and sore, more so than they had done in India. During the spring of 1888 she was very busily employed, walking about a great deal; in fact, she told me that she was more or less on her feet from nine in the morning till ten at night. In June 1888 she felt quite unable to continue her exertions. Her arms and legs felt heavy and powerless, her appetite became very bad, and she felt dull, low-spirited, and miserable. Her food, too, frequently disagreed with her. At the beginning of July she decided to take a few weeks' rest, and she then came under my care.

On examining the patient, I found her to be rather nervous. Her tongue was slightly coated with a brownish-yellow fur; her pulse was 104, full, and almost bounding in character; her temperature was 100°; respiration, 24. Her heart and lungs were apparently quite healthy; her liver was slightly enlarged, the area of dulness being $5\frac{1}{2}$ inches in the mammary line; the spleen was markedly enlarged, and extended to within $3\frac{1}{2}$ inches of the umbilicus. On examining her integumentary system, I found that a remarkable hypertrophy had taken place; this hypertrophy affected the whole of the body with the exception of the head and neck, the forearms and hands, the legs below the knees and the feet, also the anterior aspect of the trunk from 3 inches below the clavicles to the umbilicus, and the posterior aspect of the body between the scapulæ and underneath them. A reference to the woodcuts, in which the affected parts are shaded, will make the extent of the disease clear. (These diagrams are not intended to

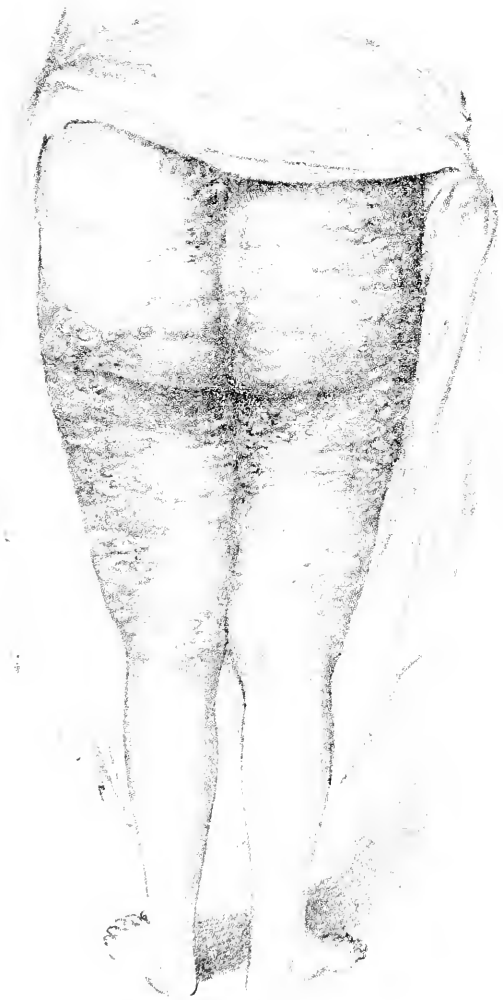
show the increase in the size of the limbs.) The upper arm measured 36½ cm. in circumference at its thickest part on the right side, and nearly 36 on the left; the thigh on the right side at



its middle third measured 71 cm., and nearly the same on the left; the calf on the right leg measured 35 cm. in circumference at its thickest part, and that of the left leg the same. There was distinctly less hypertrophied tissue upon the shoulders and beneath the scapulæ, and a deep groove separated the masses of hypertrophied tissue over the scapulæ.

There was very marked hypertrophied tissue in the lumbar region and over the lower part of the abdomen below the umbilicus; in fact, it might almost be described as a girdle of hypertrophied tissue, which sank down several inches when the patient was in the erect position. I was, unfortunately, unable to obtain any more detailed measurements, and I much regret that it was impossible to persuade the patient to consent to a proper photograph being taken of her remarkable condition. I reproduce, however, two instantaneous photographs, which may serve to give some idea of her condition after a week's treatment and her state shortly before she left my care.

The skin in the regions I have above mentioned was darkly pigmented, and formed a distinct contrast to my patient's natural complexion; it was rough, having somewhat the appearance of the rind



Dr FELKIN'S CASE OF ELEPHANTIASIS.

Before Treatment.

of a boiled orange; it was tense, and appeared to be bound down to the subcutaneous tissue; this was especially the case in regard to the upper arms, the two masses of tissue situated below each clavicle, and those over the scapulæ, but in the other regions of the body the mass of tissue had a certain amount of movement; for instance, when the patient was in the erect position, the masses of hypertrophied tissue which encircled the thighs descended, especially in front, where they completely overlapped the patellæ. On palpation, one experienced the sense of dense, brawny, slightly elastic hardness; the skin did not pit on pressure, or hardly so, but it was slightly painful to the touch. In many places, especially over the buttocks, it was distinctly nodulated, and presented a most typical elephantoid appearance, which appearance was less marked in the upper part of the body. The glands in both axillæ and in both groins were enlarged; in the natural folds of the body there was a slight exanthematous eruption. I examined the blood and the urine frequently during the progress of the case, and also punctured the tissue in various parts of the body, as I thought that at first I might have to deal with a lymphoid variety of elephantiasis; no lymph, however, could be obtained. The amount of urine passed varied from 50 to 70 ounces in the twenty-four hours; the sp. gr. varied from 1012 to 1025. With the exception of some albumen which was present in the urine for the first three weeks, nothing abnormal could be detected. On one occasion, it is true, I thought I saw a few ova of the Filaria sanguinis hominis, but repeated microscopical examination gave no confirmation. The microscopical examination of the blood gave no results, and this Dr Edington kindly confirmed; he also kindly endeavoured to obtain cultivations from the blood, but without success.

Menstruation was irregular and slightly painful.

Treatment.

Absolute rest was enjoined, and the patient had a hot bath almost every day. A moderate amount of bland food was ordered, consisting chiefly of milk (the patient could never eat fish; I tried it on several occasions, but it always caused nausea and vomiting). The patient was regularly massaged. Commencing with half-an-hour a day, the rubbing was subsequently increased to an hour and a half, and the constant current battery was applied for twenty minutes each day. A mixture was prescribed containing quinine, arsenic, iron, and strychnine, and the bowels were regulated by the frequent administration of aperients.

The progress of the case was in every way satisfactory; with the exception of a slight rise in the temperature for six days after the massage had been commenced, all went well. The patient complained of nothing excepting the battery and the pain that the massage gave her during the first three weeks. The massage and

battery were discontinued about the 10th of September, by which time the skin had become normal in character,—in fact, had returned to its usual condition, and all tension and hard feeling had disappeared. A remarkable diminution had taken place in the circumference of the limbs, the circumference of the upper arms having been reduced to 25 cm. and the middle third of the thighs only measuring 54 cm. in circumference. The mass of hypertrophied tissue encircling the body had quite disappeared. The patient stated that she felt quite well, buoyant, and fit for work. She could walk with ease and comfort, and by the middle of October she was apparently quite restored to health, and has continued so to the present time. I should perhaps mention that for a week or two after her prolonged rest the patient suffered from slight swelling of the feet and ankles. This condition, however, completely disappeared.

Remarks on the Case.

Probably the most remarkable fact in the whole case was the situation of the elephantoid growth. I have never seen a case like it before, and the one which most nearly resembles it is that of the "elephant man" which was published in the *British Medical Journal*, Dec. 11, 1886, p. 1188. In looking over the literature of the subject, I find very few instances of elephantiasis occurring, except in the feet, legs, scrotum, penis, and the female genital organs. Dr Green of Serampore refers to a very exceptional case in which the whole body of a Hindoo gentleman was affected, except the thorax and head. Dr Rose of Faridpore mentions it as sometimes affecting the abdomen. Sub-assistant Surgeon Ghosal of Bankipore has met with cases affecting the forearm and breasts of females, and in this connexion he has formed the opinion that elephantiasis is connected with either the arrival of puberty or its decline, especially as the genital organs are so often attacked, and the disease, both in men and women, often undergoes monthly variations, and in women this variation usually coincides with menstruation, and is often preceded by some derangement of that function. Baboo J. Chunder Roy of Lucknow agrees with this opinion. My patient, it will be noticed, suffered from painful and irregular menstruation, but I could gain no exact information with respect to this subject. Dr G. A. Turner, when at Samoa, reported that in 43 cases the female breast was attacked seven times. Surgeon F. M'Calmont reported that the face was affected next in frequency to the left leg, but I can find no statistics to bear out this statement. A case is reported from Dr Kerr's hospital, Canton, of a man, aged 25, who was attacked by elephantiasis on the face. His lips and nose were greatly hypertrophied, and tumours were formed under the eyes. I may also notice that Mr Saville mentions in 1873 that at the Society Islands he met with one case in which a

Dr FELKIN'S CASE OF ELEPHANTIASIS.

(After Treatment)

woman had one breast attacked and enormously enlarged. He says, too, that elephantiasis "sometimes attacks the buttocks. Only one case of this kind has come under my notice. The sufferer is a woman of about 50 years of age; the enlargement is a ponderous mass, preventing almost any act of locomotion. Both buttocks are attacked." Chevers noticed that the forearms were attacked in cases near Orissa, and says that the hands are often affected in Travancore; and Surgeon Ghosal refers to the disease as affecting the leg and arm in rare cases, and as sometimes extending to other limits. Liveing only met with one case in which first one arm and then the other was affected, both below the elbow; they were greatly enlarged. In Central Africa, where I saw a considerable number of cases of elephantiasis, I never met with it excepting as affecting the legs or genital organs.

Another point of interest in my case was the effect which eating fish had upon my patient. One of the theories of the production of elephantiasis is that the disease is at times produced by a fish diet. Clarke of Sierra Leone laid great stress on this cause; but, on the other hand, Waring says that in Travancore, where the disease is so prevalent, only 11 out of 800 cases were fishermen; and that at a fishing village near Trivandrum, containing 150 families who lived almost entirely on fish, no case of elephantiasis was seen. Now my patient's aversion to fish occurred shortly after the growth began, and it was probably only a coincidence, but I think it worth mentioning.

The next point to which I will call attention is one of considerable interest, namely, that during the whole course of the disease my patient suffered so little from fever. Waring states that there was a distinct history of fever in 99·11 per cent. of the cases he examined, and he shows from his statistics that when fever ceases to recur, increase in the hypertrophy of the tissue ceases also; but there are undoubtedly cases of elephantiasis on record in which fever has been totally absent throughout the whole progress of the disease. There is, too, some difference of opinion as to whether the fever which is present in most cases is due to a distinct fever or to malaria. Sir Joseph Fayrer says, "Even when, as occasionally happens, fever has ceased to recur, there may be a gradual but slow and painless increase in the hypertrophy." This is what happened in my case, for practically no fever was experienced and no pain was suffered by my patient, only a feeling of weight and discomfort. As a rule, in cases of elephantiasis the spleen is not enlarged, or at any rate only slightly. I think that in my case the splenic enlargement was due to progressive malarial cachexia, and not to the elephantiasis.

All authorities agree that change of climate is the most potent remedy for elephantiasis, often arresting its progress in early stages, but here once more my case illustrates an exception to the rule. My patient only weighed 9 stones 8 lbs. when she left India;

when I first saw her she weighed 11 stones 4½ lbs., so that in her case the change of climate was not beneficial to the disease.

The last point which I will notice is the effect of the treatment on the disease. I confess I was greatly surprised at it. I had hoped for a possible arrest of the growth and probably a slight diminution in size, but that so rapid a disappearance should have occurred greatly gratified me. The lady has insisted upon returning to India, and it will be interesting to know whether a return of the malady takes place.

Note.—The engraver has made the first picture appear too nodular.

Professor Simpson thought the members were indebted to Dr Felkin for his valuable paper. It would be interesting to have the opinion of surgeons or dermatologists present as to the nature of the case, which seemed in some of its features to be allied to scleroderma. He (Prof. Simpson) considered that the influence of the massage was a point of great importance, and asked if the nausea created by fish remained after the improvement in the patient's health had been established.

Dr Dods asked if there was any history of erysipelas, repeated attacks of which were declared by some observers to be a cause of elephantiasis. He had seen a large number of cases of this affection, but none like what Dr Felkin had described. He never saw a case in which the ankles were not affected. The swellings usually began in the most dependent parts, which might be expected, as elephantiasis is evidently a disease of the cellular tissue. He had seen one case in which the redundancy of cellular tissue had formed two large tumours on the back of the thighs,—one of these when removed measured 42 inches in circumference and weighed 15 lbs. It consisted of cellular tissue, dense at the circumference, and soft in the centre, with fluid in the large meshes of the tissue. In another case of elephantiasis of the face, the patient's profile resembled that of a horse. He never knew of any cases of true elephantiasis being benefited by treatment. There was often an improvement if the patient were removed from the region where the disease is supposed to prevail, viz., the vicinity of the sea.

Dr Haddon wished to know whether the improvement in the case was due to the treatment that had been adopted or to the change undergone in coming to this country. He thought the number of remedies used at the same time made it rather difficult to draw conclusions as to the cause of the improvement.

Mr Cathcart asked if massage had been used in the treatment of other cases of the disease.

Dr Felkin said that he was familiar with the scrotal tumours to which Dr Dods referred, and once removed one which weighed 98

pounds. He had also seen cases of lymphoid elephantiasis, but the present case was not one of that description. At first he thought it might be so, but although punctures were made in various parts no fluid whatever could be obtained. Massage was not recognised as routine treatment in these cases. He did not think that the tonics and battery either complicated the treatment or were unscientific. The methods employed had a definite object in view, which object was attained to a higher degree than hoped for. In this case the change of climate had no beneficial effect; although unaccompanied by pain or fever, the hypertrophy continued both during the voyage to England and after arrival there, the patient increasing some two stones in weight after leaving India. With regard to the possibility of the production of elephantiasis only on the sea coast or as far as the cocoa-nut grows, Dr Felkin did not believe the theory, as he had seen so many cases of the disease in Central Africa.

2. *Professor Annandale* read his paper on THE REMOVAL BY OPERATION OF NASO-PHARYNGEAL TUMOURS. He stated that his principal object in reading this paper was to describe a method of operating carried out successfully in three cases recently under his care. He had studied the many and various methods previously suggested and practised for the removal of these growths, and although his own method was not original in some of the details, he believed that in its entirety the procedure was new. Having freely admitted that some growths in this region might be removed by other than a cutting operation, he described the steps of his procedure as follows:—(1.) The exposure of the anterior nares by freely dividing the mucous membrane connecting the upper lip and upper jaws according to the plan of Rougé. (2.) The division of the bony septum of the nose along its attachment to the jaw. (3.) Incising the soft parts along the middle line of the hard palate, and then sawing through the alveolar margin of the upper jaw, and through the entire hard palate along the same line. The soft palate may or may not require division in its middle line. (4.) The forcible separation of the two jaws, and the introduction, through the gap of the finger, of the periosteal scraper or other similar instrument, with a view of separating the *secondary* connexions of the growth to surrounding parts. (5.) The removal of the growth from its primary site of origin by forceps, sharp spoon, cold snare, or galvanic wire. After the operation the two jaws are brought together, and retained by one or more sutures. Professor Annandale believed that his operation had for the first time demonstrated the fact, that the upper jaws, after such a section, could be separated to an extent so as to give access to the base of the skull and posterior nares. Three cases recently operated upon with success were then reported in detail.

Professor Simpson said he had seen Middeldorpf of Breslau

operate a great many years ago on a tumour, such as those described by Professor Annandale, by means of galvano-caustic wire. Mr Annandale's paper was of unusual value because of the original procedure he had devised for getting access to the root of the disease. It was new to him that so much space could be gained by splitting in the centre the palatal portions of the superior maxillæ. It was not easy at once to see how this came about, and he considered that a supplemental anatomical research would add to the value of Professor Annandale's important communication.

Mr Cathcart thought the amount of space gained depended on the age of the patient. With a full-grown hard skull, it would be very difficult to wrench aside the cut parts to such an extent, but with a young skull he thought it would be easy. A greenstick fracture, bending the wings of the sphenoid and the zygomatic arch, probably occurred. One advantage over the galvano-caustic wire of this operation was that they could be pretty certain of getting away the whole of the growth. He did not, however, know whether recurrence had been observed after the use of the wire. He asked Mr Annandale if he did not think he could accomplish all he desired by incising the soft palate alone. If he could do so, it would be a great gain.

Dr M'Kenzie Johnston said he had not noticed any reference in Mr Annandale's paper to the dangers of this operation. He should, therefore, like to ask him whether it was not such a serious one that it should only be tried after the failure of all milder measures, and after setting fully before the patient its risk? The three successful cases recorded by Mr Annandale were, no doubt, due to his skill as an operator, but in a series of similar operations recorded there were eight deaths in twenty-four cases, showing the formidable nature of the operation. By hooking forward the soft palate under cocaine, it was surprising the amount of space that could be obtained for operative procedure, and he thought no case should be submitted to the cutting operation till it had been proved impossible to remove the growth otherwise.

Professor Annandale said Esmarch and the American surgeons who witnessed his second operation were astonished at the amount of space gained. After he tried it in his first case, he asked Sir William Turner if it could be done in the dead body, and was told it could not. Division of the soft palate had been tried, and had failed to give the necessary access. His best answer to Dr M'Kenzie Johnston was the fact that his third case had been sent to him by Dr M'Bride for operation. He believed that in the majority of these cases a cutting operation would be found necessary.

3. A CASE OF TUBERCULAR ULCERATION OF THE BLADDER, WITH UNUSUAL CLINICAL HISTORY.

By R. W. Philip, M.A., M.D., F.R.C.P. Ed., Physician to the Victoria Dispensary for Consumption and Diseases of the Chest, Edinburgh.

Tuberculosis of the bladder is a condition of so comparative infrequency clinically that I have thought the following history worthy of record. The case itself presents some other features of unusual interest.

The patient, P. P., aged 18, first came under my care on the 18th December 1883.

At that date there was a history of hæmoptysis, some one and three-quarter years before. The hæmoptysis had not recurred till two days before I saw him, *i.e.*, 16th December 1883, when he brought up a considerable quantity of blood. The patient described it as "coming up in mouthfuls." Beyond this the previous history, as reported, was satisfactory. The family history was fairly good. There was no evidence of phthisical taint.

Examination showed the patient to be about 5 feet 8 inches, slightly built, and of spare habit. The complexion fair, bright, and clear. There were no manifest signs of disease. Patient was thin, but there was no marked loss of flesh. There had been no night sweats. Previous to the later hæmoptysis there had been but little cough. There was slight dyspnœa. The chest showed slight flattening below the right clavicle, and over the same area and a corresponding area posteriorly the note was relatively duller. Auscultation anteriorly from the apex to the level of the third right interspace, and posteriorly to the middle of the scapula of the same side, revealed the presence of bronchial breathing, largely marked by medium crepitations, with increase of vocal resonance. The left side was practically normal, with the exception of general feebleness of the respiratory sounds and slight prolongation of expiration.

Expectoration was not copious. The sputum (examined by the Ehrlich-Weigert method some weeks later) was found to contain the tubercle bacillus. The patient's appetite was feeble. The urine showed a faint trace of albumen, but in respect of this last observation it should be noted that the patient, as afterwards appeared, had been for some years up till that date a persistent masturbator. The urine was not altered in respect of quantity or specific gravity. Such was the condition in December 1883.

With regard to the treatment and progress of the case, it is unnecessary for the present to say more than that during the succeeding months the patient made steady progress, and at the end of spring he had gained in weight; the expectoration, which had never been very copious, gradually disappeared, hæmoptysis did not

recur (with the exception that on one or two occasions the sputum was streaked with blood), the crepitations became less marked, and the cough had gone.

At the commencement of the winter of 1884 the patient was examined with care, and the following points noted:—There was an increase of more than 6 lbs. of weight. The whole right chest, but more particularly the apex, was much retracted,—the sternal end of the right clavicle projecting anteriorly, so that it could be felt as a rounded prominence through the clothes. The apical dulness remained. Breath sounds over the dull area were hardly audible. Morbid accompaniments of the breath sounds were absent. On the left side the condition was little altered. The lung appeared healthy, with slight accentuation of the respiratory sounds. There was practically no expectoration, and the cough had quite gone.

Throughout the winter this progress was maintained, and in the spring of 1885 his condition was so satisfactory, that I allowed him to resume office work. The patient did not see his way to go abroad. For about two and a half years from this date he continued hard at work in an accountant's office, with few interruptions on account of passing colds or slight gastric disturbance. He seemed perfectly restored, with the retracted lung as the only remembrance of this former illness.

Last winter—that is to say, the third winter since he had resumed business—he grew less careful of himself, more especially in respect of evening work and engagements. Occasionally he was laid up with slight colds, but these he threw off easily, and the chest, examined from time to time with care, remained undisturbed. He was apparently in good health till last June (1888), when he came to see me on account of having passed what he thought to be a little clotted blood, after making water. Apart from this there was no symptom. Unfortunately, he had not kept the clot, but his water was examined, and found to contain no albumen, no blood, no sugar. There was no polyuria or frequency of micturition; nor was there any pain during the act, or on pressure over the pubes. At my request he came to see me again in a week's time, with various samples of urine, and in one of these a small piece of shreddy, almost pultaceous, substance was found, and a few blood cells. At this date, while he did not complain of further lung symptoms, a few distant crepitations could be heard by auscultation over the second and third interspaces of the right side. After that he passed repeatedly, but often at intervals of some days, similar masses. From the first I examined these for the tubercle bacillus. They appeared to consist largely of round cells and structureless material. The tubercle bacillus was not at first discoverable. After many attempts, one mass was obtained which examination showed to be crowded with the bacillus, so that no possible doubt remained as to the diagnosis.

Some weeks later the patient caught a severe chill after being out late, which appeared to aggravate the lung condition. Cough returned, and abundant crepitations speedily appeared in the right, *i.e.*, the already retracted side. At the end of six or eight weeks, the previously healthy lung became affected. Slight impairment of the percussion note was traceable, and a few crepitations were heard in the second and third interspaces anteriorly. The patient rapidly lost flesh. The temperature, which during the whole course of his illness, apart from the early hæmoptysis, had been seldom elevated, now fluctuated between 99° F. and 102° F. Within six weeks or two months all the ground which hitherto had been gained was lost.

When the shreds were first discovered I examined *per rectum*, and was able to trace slight tenderness on pressure towards the neck of the bladder, and some fulness to the left of the middle line in the region of the left seminal vesicle. Mr Caird, who kindly saw the case along with me some weeks later, confirmed the latter observation. There was no apparent disturbance of the testicle or prostate. There was no clear evidence of kidney affection.

I concluded, therefore, that the source of the membranes, which were discharged intermittently, and some of which, of irregular shape, were about the size of a ripe pea, must be the bladder, while the other parts of the genito-urinary tract were not necessarily affected. Evidence of other cases gave presumptive evidence that the kidneys possibly shared the disease. The patient gradually sank, and died on the 4th of December 1888.

On *post-mortem* examination, the right lung was found to be small, and bound by extremely firm adhesions to the costal wall. The lung was of firm consistence and tough to the knife. The cut section revealed a dense fibrous structure, with some scattered tubercle. The lower part was less fibrous, and tubercles were more abundantly evident. There was one vomica about the size of a small bean. The left lung was slightly adherent towards the apex. There was no pleural effusion. The lung was firmer than normal, and in parts emphysematous. On cut section, it was found congested and partially studded with what appeared to be recent tubercle, located more particularly in a band of about $1\frac{1}{2}$ inches broad running midway across the upper lobe.

There was no fluid in the abdominal cavity. The right kidney, of peculiar S shape, seemed normal, also the right ureter. The left kidney was enlarged, and evidently tubercular (but this was not cut into); the left ureter was thickened and considerably dilated. The bladder showed a small erosion at the entrance of the left ureter, and a large ulcer, about the size of a florin, $\frac{1}{4}$ inch deep, situated on the anterior aspect of the bladder, near where the wall becomes continuous with the neck. In the centre of the ulcer the bladder wall had almost given way, while the

edges were irregularly excavated, with shreddy fragments loosely attached. Apart from this, the bladder wall was uniformly smooth, and not inflamed. The prostate and urethra appeared normal. Both seminal vesicles were thickened, and the vas deferens of each side, as also the duct, was thickened and hard. The testicles seemed healthy.

The case appears to me one of peculiar interest, in respect more especially of the following points :—

(1.) It illustrates the extremely protracted course of some cases of tuberculosis pulmonum (the patient was five years under close observation) and the treachery of apparent cures.

(2.) The most hopeful line of spontaneous cure is that of fibrous formation and gradual shrinking of the affected lung.

(3.) There may be a comparative absence of symptoms at the onset of tubercular affection of the genito-urinary system.

(4.) In relation to the gravity of the bladder lesion, the persistent absence of cystitis or even discomfort in micturition is remarkable. The passage of so large membranes without the patient's knowledge is noteworthy. The patient was never conscious of their being passed till they were seen in the chamber-pot.

(5.) The probability of the source of the membranes in the bladder was presumed from their size and irregular form. (This was confirmed by *post-mortem* examination.)

(6.) The diagnosis of their tubercular nature was rendered absolute by the Ehrlich-Weigert method of staining and microscopic examination.

(7.) Some difficulty was experienced in connexion with the staining process. Many membranes were examined before bacilli were detected, while the individual bacilli, when stained, took on the stain less readily than tubercular bacilli obtained from other sources.

(8.) A possible relationship might be suggested, though I am not inclined to lay a great deal of stress on this, between the previously long-continued masturbation and the determination of the tubercular process to the presumably weakened system.

Professor Simpson said that in the female one might not unfrequently see the correlation of tuberculosis of the genito-urinary system and lung disease, but there had always been marked dysuria in all the cases that had come under his observation. He never saw a case with such an absence of symptoms as described by Dr Philip.

Professor Annandale was glad that Dr Philip had described this case of tubercular disease of the bladder so carefully, because it was a subject about which the books said very little. He could recall four or five cases in which after opening the bladder for chronic cystitis, he had come upon ulcerations of a similar character which he had believed to be tubercular.

Dr James Ritchie was struck by the absence of symptoms, and asked what was the condition of the young man's nervous system. Was he one who bore pain readily?

Mr Cathcart thought the absence of cystitis partly explained the absence of pain, but the position of the ulcer was also important. When the ulceration affected the base of the bladder or extended around its neck the symptoms were distinct, but high up the symptoms were mild or absent altogether. In Dr Philip's case the larger ulcer lay on the front of the bladder above the urethral orifice, and the smaller was near to the orifice of one of the ureters. This, however, was a matter requiring further investigation.

Dr Aitken considered that the condition of the ulcer itself might account for the absence of symptoms, if covered with pultaceous material.

Dr Philip agreed with Mr Annandale as to the paucity of reference in literature to tubercular ulceration of the bladder. This had been one of his reasons for recording the case. He thought that cases such as those Mr Annandale mentioned should have the urine examined for the tubercle bacillus. He presumed that if evidence was obtained of a tubercular lesion in the bladder, Mr Annandale would not operate.

Professor Annandale.—But I would certainly operate.

Dr Philip, continuing, said perhaps in the circumstances the patient would not care to have the operation performed. In any case, it was desirable to obtain an absolute diagnosis. In respect of the absence of urinary *symptoms* he thought the case peculiar. This was one of the special points which the paper illustrated. As to the cause of this absence, Mr Cathcart's view seemed to him of value. It was particularly to be noted that apart from the large and small ulcer the vesical mucous membrane was intact and perfectly smooth.

Meeting V.—February 6, 1889.

Dr John Smith, *President, in the Chair.*

I. Election of Ordinary Members.

John Smith, M.D., 16 Townsend Crescent, Kirkcaldy; Allan C. Sym, M.D., 144 Morningside Road; and Edmund Price, M.B., C.M., 28 Mayfield Road, were elected Ordinary Members of the Society.

II. Exhibition of Instruments.

Professor Annandale exhibited instruments—(1.) In connexion with Intubation of the Air-passages. (2.) A new bone-drill.

III. Exhibition of Specimens.

1. *Mr A. G. Miller* exhibited a SIGMOID FLEXURE which caused death by volvulus. The patient, M. H., æt. 18, had her bowels moved on 31st December twice. Previous history of constipation and occasional colicky attacks. On 1st January seized with pain suddenly, referred to umbilicus, and very severe. Vomiting occurred almost at once, and frequently on way home; felt better after a while, and went out to a party. Constipation persistent; vomiting ceased after 3rd January till 9th January, never fæcal or severe; abdomen distended and tympanitic, and slightly sensitive. Injections did not pass beyond rectum. Nothing to be felt by finger in rectum. No appearance of collapse till 9th January.

Treatment.—Opium-meat suppositories; rectal enemata.

Operation on 9th January 1889. Incision from umbilicus to pubes; no protrusion of bowel. Sigmoid flexure greatly distended and lying over into right lumbar and iliac regions; also twisted on itself quite half a turn.

Explanation of Mechanism of Twist.—Sigmoid mesentery unusually long and narrow. Bowel somewhat constricted at junction with descending colon and with rectum. At latter point is a distant fold with commencing stricture. Obstruction was at once relieved by undoing the twist. Bowels acted freely, colon emptying itself while patient on table. Patient did well for two days. At 10 P.M. on Friday the 11th, patient was suddenly seized with pain, became collapsed, and died before midnight.

Post-mortem on 13th January.—No peritonitis. Obstruction re-established with great distension of sigmoid, which was doubled back on the descending colon.

2. *Dr Lundie* exhibited—(*a.*) UNITED FRACTURE OF NECK OF FEMUR. The patient, a lady, then aged 71, met with an accident about four years ago, by falling on the carpet in her room. Immediately there was pain and powerlessness of the limb. Dr Duncan and he (Dr Lundie) saw her. There were all the usual symptoms of unimpacted intracapsular fracture of the femur. They decided to put her up with long splint and extension. After six weeks the bone appeared to be firmly united. He was sorry afterwards he did not leave the limb perfectly free after that; for to make assurance doubly sure he put on a starch bandage; and while it was on the patient had a bad carbuncle, and after recovering from that, was a considerable time in regaining the power of the muscles of the limb. In the course of a year or so she had as good use of the limb as she had had before. That continued till a short time before her death. He was fortunate enough to get a post-mortem, and removed the specimen. He was not prepared to commit himself as to where exactly the

fracture had been. It looked to him as if it had passed through the upper surface of the neck. The neck had certainly been very much shortened, and on the articular surface of the head they would see an indication of change of angle between the neck and the shaft of the femur. The head had been previously set in more at right angles than it is at present. If the fracture had been an impacted one, certainly it had not any of the symptoms of it at the time. Specimens were also exhibited from the same patient, showing how a simple tumour may lead to a malignant one. A hard tumour, recognised during life, was found at the post-mortem to be a scirrhus developed in one (right) of two cystic ovarian tumours. (*b.*) FISH BONE REMOVED FROM THE ŒSOPHAGUS. Dr Lundie said there was nothing remarkable about the bone (the clavicle of a haddock, $1\frac{3}{4}$ in. long). It had, however, shrunk somewhat, and did not look so formidable as when brought out. The reason he showed it was that he might call attention to a serious imperfection in the instrument by which it was removed. He was sent for to see a young woman who had been eating some fish in a great hurry, when she felt the bone stick in her pharynx. She swallowed a piece of bread to try to get it down; and after doing so felt a sharp pain at the level of the xiphoid cartilage instead. It seemed to him that the bone had wounded the lower end of the œsophagus; and as the pain had come on each time she had swallowed for nearly thirty hours, he thought there was a strong probability that the bone was still there. He intended to try to remove it with an expanding probang, but found that while the œsophagus is nine or ten inches long, the instrument was only eleven inches, so that there was only an allowance of one or two inches for mouth and pharynx. Dr Aitken's instrument was the same as Dr Lundie's, and he suggested an extemporized addition to it. The bone was brought up. It seemed curious that makers should go on from generation to generation making an instrument four or five inches too short to effect completely the purpose for which it is intended.

IV. ORIGINAL COMMUNICATIONS.

1. ARTIFICIAL LEGS.

PART II.—SITES FOR AMPUTATION IN THE LOWER LIMB IN RELATION TO ARTIFICIAL SUBSTITUTES.

By CHARLES W. CATHCART, M.B., F.R.C.S., Eng. and Edin., Assistant Surgeon, Royal Infirmary, Edinburgh; Lecturer on Surgery, Edinburgh School of Medicine.

IN a former paper I endeavoured to show that, in spite of a consensus of opinion by instrument makers to the contrary, in amputations of the foot, and so far as after usefulness was con-

cerned, there was every reason why surgeons should save as much of the natural parts as the nature of the case permitted. Moreover, from a study of the physiology of locomotion, it seemed to follow that the principle of "the least sacrifice of parts" in the lower limb ought to be as applicable to modern surgery as it ever had been before. The justifiability of removing—with the view to an artificial limb—more of the natural parts than would have been otherwise necessary was not considered, because the advantage to be gained by so doing did not seem to me to have been established.

It remains still to be considered how far from practical as well as from theoretical considerations the same conservative principle may apply to the leg and thigh. Our first duty will, therefore, be to inquire whether instrument makers really make the same demand on surgeons for the leg and thigh that they do for the foot. One maker,[1] at least, who has recently published his views very widely, speaks with no uncertain sound. "If," he says, "the heel cannot be saved, save the knee-joint and its due length of stump, and amputate somewhere in the middle third of the lower leg (tibia); and if the knee and its functions cannot thus be preserved, amputate as low in the middle third of the thigh (femur) as possible." As a commentary upon these rules, we may quote other passages from the same author. Thus, under the head "Amputations through the Tibia," he says, "It is obvious that one must not amputate too low, because a certain amount of space is required for the ankle-joint mechanism (and this space is at least about four inches from the ankle-joint or six inches from the heel). It is also obvious that if the amputation is performed too near the knee, then the stump left below the knee is of little or no service in governing its portion of the artificial leg; nay, further, a very short stump below the knee almost invariably contracts in a flexor direction, and becomes, therefore, a great inconvenience rather than the reverse. In an adult, for the stump to be of any service, at the very least four inches below the knee should be left. The absolute range of election is therefore from four below the knee to four above the ankle, but, practically, it is about the middle third, and the lower in this the amputation is performed, the more perfect will be the government gained over the artificial leg. Consequently the actual point of election will be a little lower than half way down the tibia; while if it is impossible to leave a stump up to four inches length, then it is perhaps better not to amputate through the tibia at all, but higher up" (pages 23, 24), *i.e.*, four inches from the condyloid surfaces of the joint.

There can be no doubt as to what is here meant. As in the foot, so in the leg and up to the lower third of the thigh, surgeons are to give up "least sacrifice of parts," in order partly to give room for the mechanism of artificial joints (above ankle and

[1] *Artificial Limbs*, by Heather Bigg, 1885.

knee), partly to facilitate control of artificial limb by stump (below knee), and partly lest the stump should be not only useless but "a great inconvenience" (below the knee). So sweeping a demand is almost its own condemnation, but, as it is made in the name of large instrument-making experience, we must give it careful consideration. There seem to me to be four points of view from which this question may be considered:—

(1.) From the theoretical or *à priori*, *i.e.*, from a study of the physiology of locomotion.

(2.) From that of the author, by considering his own arguments in defence of his position.

(3.) From that of other instrument makers.

(4.) From that of personal experience.

1. *Physiological Inquiry.*—This I think I have already stated[1] with sufficient fulness, when I showed that in the knee, no less than in the ankle, there were movements in the passive, but specially in the active phase[2] of the step, which, when lost, no

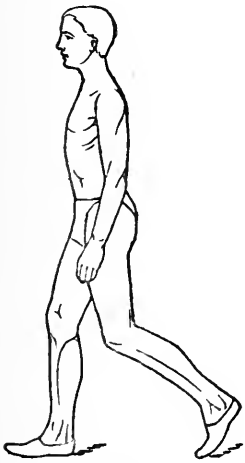


Fig. 1.

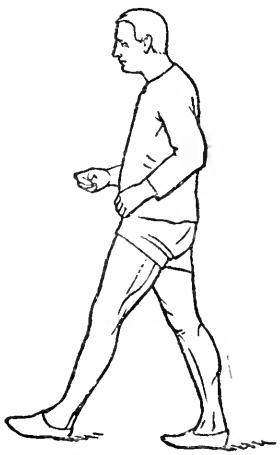


Fig. 2.

mechanism could replace, and that the longer the stump of the lower limb as a whole, the more the leverage and the greater the propelling power of the thigh, even when the stump below the knee was too short to allow it to control the artificial limb.

2. The chief reason given for urging surgeons to leave so much as 4 inches above the knee and ankle is that without this space the rule-joints instead of the pivot or hinge joints would be required, and that rule-joints are much inferior to the others.

[1] "Artificial Legs," Part I., vol. vii., page 15.

[2] The slightly flexed knee, as the body passes over the supporting leg (see Fig. 1), and the combination of an extended knee at one time with a flexed, and at another time with an extended ankle-joint (see Fig. 2).

From a calculation based on the breadth of weight-bearing surface Heather Bigg alleges the inferiority to be as 1 to 43. Assuming this to be theoretically correct, and considering first the ankle, it is difficult to see how so much as 4 inches of space above the ankle is called for when the ankle mechanism for a Syme's or a Pirogoff's stump is not only provided, but is called "one of the most perfect in the range of mechanical appliances" (page 45).

Why might the ankle movement provided for one of these two operations not be supplied for an amputation higher up? The difference in bearing will surely not explain it, because this maker often takes the bearing of a Syme or of a Pirogoff at the ischium.

But, again, these rule-joints, bearing at the knee nearly the entire weight of the body, are provided by this maker for all amputations above those at the ankle, since he holds that for every amputation above a Syme or a Pirogoff the bearing of "the artificial limb should be entirely taken on the very top of the thigh, and more particularly on the ischial tuberosity or sitting bone." "The artificial limb must reach to the ischium, or, in other words, must enclose and embrace the full entirety of what part of the natural limb is left from the buttock downwards, . . . and so transmit the weight of the body directly from the pelvis to the ground."

Whether this plan is or is not necessary we need not here discuss, but since this maker must use these joints in a large proportion of his artificial limbs, it seems strange that to get rid of the need for them in certain other cases he should ask surgeons to remove 8 inches of a patient's lower limb more than would have been otherwise necessary. In addition to the above, more than one dealer in artificial limbs has told me that he finds very little difference between the lasting powers of rule and of the other form of joint.

3. *Other Makers.*—In the series of questions which I put to instrument makers I specially framed five to bear upon the present discussion relative to the knee. (The distance thought necessary to leave above the ankle I unfortunately omitted to ask.) We may divide the inquiry, meanwhile, into amputations below and above the knee. If we turn to the answers to these questions, we shall find that the opinion of instrument makers is much more divided upon them than it was upon the advisability of sweeping away the whole foot if part were destroyed.

The two questions bearing on amputations through the upper part of the tibia were—"What length of stump below the knee do you find it necessary to leave in order that the movements of the artificial limb may be controlled by those of the stump?" and, "Does the seat of election give too short a stump for this purpose?" To the first of these questions, *i.e.*, No. 2 of the series, the answers varied. IV.[1] replied 4 inches; 5 inches was the reply of III., VI.,

[1] See list of Instrument and Limb Makers at end of paper.

VII., and VIII.; 5 or 6 inches of II.; the junction of middle and lower third of leg of I. and V.; 8 inches from patella of IX. XI. replied, "Never amputate lower than 2 inches above ankle-joint, and never leave less than $2\frac{1}{2}$ inches of tibia below patella."

Indirectly, in replying to this question, several gave answers bearing also on the next question. Thus IV. says, "I have fitted patients with only 3 inches below the knee and they walk well." VII. says, "A serviceable limb can be applied to any stump, but unless 5 inches are left below the condyles of the knee we often have to insert springs or other mechanism to enable the wearer to control the movements of the leg." X. says, "We can apply a leg to a stump $1\frac{1}{2}$ inches below the popliteal space, and use the stump in controlling the knee movement of the artificial leg; this, however, is somewhat short, and cannot be expected to do the work of a longer stump."

To the question with reference to the seat of election, I. replied, "Yes, it necessitates a kneeling leg;" II. and III., "Yes;" IV. implied "Yes;" while the answer of V. was not to the point. XII. replied, "Never leave less than $2\frac{1}{2}$ inches of tibia below patella;" on the other hand, VI. reported that he had often successfully fitted such cases. The answer of VII. has already been partly given; he also here stated, "Yes, for effective control," but would recommend going from middle third of leg to "through the knee." VIII. said that "the seat of election is rather too short, but we have some very good cases of that kind." IX. said, "In all cases it is very desirable to save the knee-joint, even if the stump is but 2 inches long. In that case an artificial limb can be so adjusted that the patient will walk very nicely."

It would have been better had I definitely asked if above the knee would have been preferable to less than 4 inches below it; but the answers brought out by the question as I put it seem clearly enough to show that many makers are prepared to deal with any stump, however short, below the knee.

Since the bent knee is so well adapted for bearing pressure, one naturally asks if with a short stump, in which, either from stiffness or extreme shortness, the knee-joint cannot be utilized, an artificial limb with movable knee cannot be adopted in which the "bearing" is taken on the flexed knee. This, I find, is much desired by many of the makers, and is spoken of in their books descriptive of their limbs as quite an usual appliance. As an example, X. refers thus to one of his figures: "Fig. 3 is a knee-bearing leg. It is to be applied where amputation takes place below the knee, and where the stump is too short or contracted at right angles so the knee-joint cannot be used in walking." Other makers express a similar opinion. II. also writes: "In a case of amputation at seat of election it is best to bend the knee to a right angle, and have the artificial

leg arranged with a socket in which the stump would rest front downwards, the leg having lateral knee-joints; the stump would thus project backwards." Hence we may conclude that even should the stump be too short or too stiff to control the artificial limb, the "knee-bearing" and also longer stump will be preferable to a higher amputation.

4. I have myself seen stumps of "seat of election" amputations admirably fitted with artificial limbs with movable knees, but have not as yet seen any with kneeling bearing. When such is employed I would, on general grounds, consider that the best results would be attained by insuring that the thigh and not the leg-piece of the artificial limb took the "bearing." See Part I., vol. vii., page 28.

Above the Knee.—When no portion of the tibia can be saved, the surgeon has to choose between amputation through the knee-joint, through the condyles (Carden or Gritti), or at sufficient distance *above* the joint to permit of a hinge-joint knee mechanism. What are the advantages and disadvantages of each? Considering this question under the same heads as before—1. Physiological, the need for leverage is as much seen here as in any of the places already noticed. 2. The contention for the higher amputation is that sufficient room is not left for an artificial knee-joint; but as rule-joints have already been shown not to be such a great evil, this argument does not count for very much after all, especially as I shall afterwards show that the only other possible mechanism—tendo Achilles—for which room might be required, carries no real advantage with it. 3. Other makers. My questions to instrument makers on these points were as follow:—(*a.*) What distance *above* the knee-level should be left by the stump in order to give room for the mechanism of an artificial knee-joint? (*b.*) Do you find any difficulty in fitting a good substitute limb to the stump of a Carden's amputation, or do you prefer the stump of an amputation through the thigh at the lower third? The answers to these will be best taken together, as they throw light on the other. Taking first those makers who prefer *amputations above* the knee-joint:—

I. answered (*a.*) Fully 3 inches above the centre of the knee-joint. (*b.*) We can fit a good substitute limb in a case of Carden's amputation, but it requires outside steel joints. We prefer, therefore, the amputation to be at the lower third of the thigh, as it allows of a solid joint, which is lighter and neater.

III. answered (*a.*) It should be 3 inches. (*b.*) Carden's amputation is unsatisfactory where a knee-joint is required, and we have no difficulty in applying an artificial leg with ankle and knee joints when amputated at about lower third.

IV. answered—The artificial limbs we supply will allow of 1 inch above the centre knee, though we prefer amputation about the middle of femur. The amputation at knee-joint is not good; we make an artificial leg for this amputation. The upper bracket

must be longer than the sound leg, or it can be made the same length by putting steel joints at sides of knee—a great disadvantage, as the rivets of knee-joint soon wear loose and rattle. The amputation through the thigh at lower third is a good amputation for all kinds of artificial substitutes.

No. V. (*a.*) "In the case of a man 5 feet 7 inches, about 4 inches above the knee. (*b.*) For mechanical reasons, amputations through the joints prevent the mechanism from constructing an efficient or durable joint. . . . For working man, with no desire to bend his knee, a Carden's amputation is admirable."

No. VI. (*a.*) "Five inches quite enough. (*b.*) My favourite operation is just above the condyles; and had I to be operated upon, would select this operation before lower third of thigh, or even seat of election."

No. VIII. (*a.*) "Four and a half inches. (*b.*) I prefer the amputation through thigh at lower third; in my opinion, the best walkers are amputated there (both above and below the knee respectively)."

No. IX. "Amputations through the knee-joint are undesirable, for the fact that the remaining portion is usually very sensitive to the touch of anything; also being larger at the circumference than at a point 2 inches above, it necessitates a very ugly-looking artificial, and gives more or less annoyance to the wearer. An amputation 2 inches above the knee-joint leaves sufficient room for the artificial knee. Notwithstanding these objectional cases of amputation to which I have alluded, we adjust artificials to them in such a manner that the patient walks very well."

No. XII.—"Never amputate through the knee-joint, but 3 inches from the knee-joint. An amputation through the knee, or only removing tuberosity of femur, renders me incapable of fitting it with either a leg for above or a leg for below knee amputation. I have no room for forming an artificial knee-joint nor any lower stump to articulate lower part."

Three of the answers, however, were favourable to *amputations through or close to* the joint. No. II. writes, "It is not necessary to leave any space between the knee-joint and stump for mechanism, because if the amputation was through the joint or just above it, the artificial joints would be arranged to go outside or on each side of the end of the stump."

No. VII. says, "We find no difficulty whatever in fitting a limb to any case of amputation from the knee-level upwards, providing some stump is left. In our designs we modify the artificial knee-joint according to the point of operation. When there is no section of the bone shaft, a bearing can be obtained over the end of the stump, especially if there is a good flap well brought up out of the way. When this is the case, much less weight is borne laterally by the stump, and the usual wasting of the adipose and muscular tissue to some extent lessened. Both Carden's and

Pollock's amputations are excellent. It is certainly an advantage to leave the patella if well covered and firmly disposed.

No. X.—The advantage of having the end of the femur to bear upon more than offsets the disadvantage of the extra elongation of the artificial thigh in order to place the mechanism of the knee below the stump; therefore we advise the disarticulation of the knee. If the amputation is to be made above the knee articulation, there is no advantage in amputating less than about 3 inches from the knee-level. We are not in favour of trimming the condyles in knee-joint amputations. We rather prefer them in a healthy stump, as they afford us means for securing the leg to the stump with less shoulder suspension."

As the weight-bearing capabilities of stumps form an element in the varying opinions just quoted, it may be well here to quote my next question, and the answers to it. It ran thus: "Can you frame any general conclusions as to the frequency with which the 'bearing' can be taken on the face of a stump, or upon the limb just above it?"

No. I. "We find it very seldom that the whole weight can be borne on the face of the stump, but we often put in a pad to bear a portion of the weight."

No. II. "It is not often that pressure can be borne on the end of the stump. Syme's amputation admits of it. Amputation through knee-joint also allows of the weight being borne to a certain extent on end of stump aided by support at top of socket."

No. III. "It is very seldom that patients can bear any pressure on end of stump for at least twelve months after operation, but any amount of pressure at buttock or tubercle of tibia."

No. IV. "It is my general practice not to take any bearing on the end of stump; there is only one that I know out of hundreds that have been under my care that can take his bearing on end of stump, and that only partial. The principal bearing being taken round thigh, the stump becomes tender and sore, but to take bearing on the tuberosity of the ischium."

No. V. "No artificial limb can be constructed so as to safely permit pressure upon the end of the bone. The art consists in adapting mechanical aid to surrounding parts so as to relieve the end from all pressure. The stump should bear most pressure in thigh and leg amputations."

No. VI. "Nearly every Syme's, and when through the condyles."

No. VII. "Wherever it is absolutely necessary to saw through the bone shaft, no reliance can be placed upon the possibility of taking the weight upon the end."

No. VIII. "Except in a Teale's or a Syme's amputation, I have the bearing brought *around limb*, though occasionally the pressure may be divided between face of stump and the limb above it."

No. IX. does not refer to this point.

No. X. "In disarticulations (either ankle or knee) we can almost invariably bear on the face of the stump, but in amputations through the shafts of the bone, pressure can rarely be taken on the face, only in cases where the extremity of the bone has a good periosteal covering, and the flaps are not adherent to the bone."

If we now sum up the discussion on amputations through or above the knee, we find that only a certain number of artificial limb makers agree in calling for amputations well above the joint, and that their reasons are—

(1.) Want of room for mechanism.

(2.) Clumsy appearance of the new limb, which must go outside the stump.

Against this view we find other makers holding—

(1.) That the difference in mechanism is not of much importance.

(2.) That any such disadvantage that may exist is more than compensated for by the increased bearing power of a bone section through an articular end, and by the better grasp that the new limb can be made to take above the broadened articular extremity, and so to allow shoulder suspension in part or completely to be dispensed with.

4. Personally, I have had no experience of stumps at the knee-joint or just above it which have been fitted with working knee-joints. But from the statements of the makers quoted, from the usefulness of rule-joint knees in lower amputations, and from *à priori* considerations, I can see no reason to depart from the conservative principles already laid down.

Before closing this paper, I wish briefly to consider some of the details of mechanism of artificial joints for which limb-makers ask us to sacrifice so much.

1. *Rule or Side Joints* v. *Joints with a Transverse Axis.*—Here I would be willing to grant some advantage to the latter, but, as already stated, not an advantage which may be taken into account.

2. *Artificial Ankle-joint.*—Very much labour and ingenuity has been expended over various forms of artificial ankle. I cannot see that even the best possible form of artificial ankle-joint has any advantage except in appearance over the curved sole and rigid foot-piece essentially introduced by Count Beaufort. Inventors who pride themselves upon the exactness with which they have imitated the natural ankle-joint, forget that the chief features of the natural joint are the muscles and tendons which actively control its movements.

The trajectory of the knee in natural walking is nearly in a straight line (Fig. 3, G G_1), and an examination of the movements which bring this about show that a varying combination of movements between the knee, ankle, and foot are needed, such that no mechanism can imitate.

If we consider, however, how else the straight line trajectory of the knee could be brought about besides by active living joints, we will easily see that a curved sole would do it.

If the foot roll on an arc of a circle whose centre is at the knee-joint, it will be easy to see that the knee will be kept as steadily in a straight line as the axle of a wheel would be; the length of step will be less, but the evenness of gait will be much the same as in nature, and this has been found in patients wearing the Beaufort leg.

If we contrast this with what occurs in the artificial tendo Achilles legs, we will find a very different result. The two com-

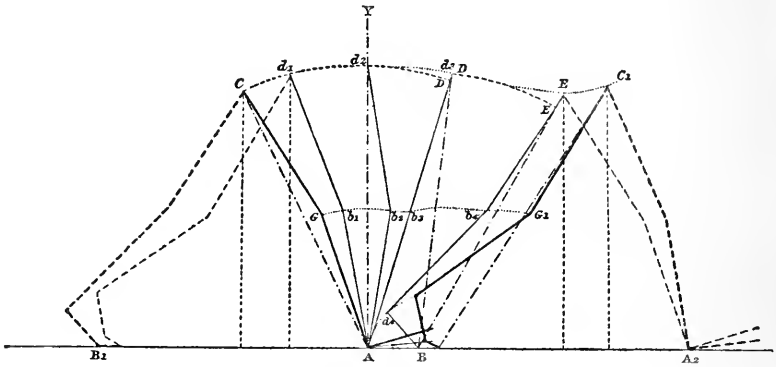


Fig. 3.—After Professor M. Marey's diagram in his paper, "Analyse cinématique de la marche," *Comptes rendus*, 19th May 1884.

binations possible and invariable with this mechanism are the simultaneous flexion and extension of the knee and ankle.

The combined flexion is useful for clearing the ground as the limb is being carried forward after one step to begin the next, but the combined extension of the two joints necessitates an unnatural trajectory of the knee, and thus a considerable disturbance of the gait. While the artificial foot is on the ground, the whole limb pivots round a centre corresponding to the balls of the toes.

If, on the other hand, there be a movable artificial ankle-joint without any tendo Achilles, the pivoting occurs at this joint instead of at the ball of the toes.

In either case the knee must pass through an arc of a circle (whose centre is at the ball of the artificial toes or at the ankle respectively), and the gait be altered accordingly.

Many limb-makers pride themselves on a lateral movement of the foot. This some of them ignorantly put at the ankle-joint; but even when it is put, as a few do, mid-way in the foot, I am doubtful if the gain is as great as is alleged.

Putting these points together, there is no real advantage in walking to be gained by making the artificial knee and ankle

joints work together by a tendo Achilles—therefore no room need be left for any part of this mechanism above the level of the knee. Similarly at the ankle, mechanism for a complicated joint need not be allowed when a simple curved sole will do as well and better than any artificial joint. Moreover, if lateral play of the foot be wanted, the mechanism for it might surely be provided for in the foot itself.

Since the curved sole does not look very well, I am trying to devise a foot with a curved sole the fore part of which will flatten out, when not pressed on, to save appearances.

I am greatly indebted to those makers of, and dealers in artificial limbs who have so kindly replied to my questions, and I now append a—

List of Authorities referred to.

1. H. Hilliard & Son, 7 Nicolson Street, Edinburgh (Dealer).
2. Archibald Young, 57 and 61 Forrest Road, Edinburgh (Dealer).
3. J. Gardner, 32 Forrest Road, Edinburgh (Dealer).
4. K. R. Schram, 6 Princes St., Cavendish Sq., London (Dealer).
5. J. Critchley, 88 Upper Pitt Street, Great George Street, Liverpool (Maker).
6. J. Scotland, Springburn, Glasgow (Maker).
7. M. Masters & Sons, 210 New Kent Road, London (Maker).
8. D. E. Corcoran, 124 Stephen's Green, Dublin (Maker).
9. W. J. Stickle, 658 Broadway, New York (Maker).
10. A. A. Marks, 701 Broadway, New York (Maker).
11. H. J. Stump, 53 Bolsover Street, Great Portland Street, London (Maker).

Dr Joseph Bell said that the Society was much gratified with the paper. While not agreeing in every respect with the conclusions of the previous paper, he entirely agreed with everything Mr Cathcart said with regard to the conservation of parts. It struck him that one of the instrument makers had grasped the root of the matter. It gave him the impression that instrument makers could do almost anything with the stump if the cicatrix was well out of the way. When he alluded to the cicatrix being well out of the way, he seemed to know how important it was for the flap to have no visible cicatrix. If you get a flap with a well-healed unattached cicatrix, and if you get the cicatrix out of the way, either behind or front, so as not to be pressed upon by the places in the artificial limb where the weight is borne, then it does not matter, and you ought to save as much as possible of the patient's body. He thought that if the cicatrix were kept off the bone, the instrument maker was able to make a limb which practically was very nearly if not equally as good as the sound one, but it was necessary that the cicatrix should be perfect and loose. Dr Bell then

referred to his method of amputation in lower third of the leg by a long anterior flap, and alluded to a case of a man treated twenty-five years ago, who was till lately working as a railway porter with a limb which could not readily be distinguished from the sound one. He had never been asked to amputate any lower limb above the ankle because the flap seemed too long. He had often amputated a limb because the cicatrix had been either adherent or was in a bad position.

Professor Annandale thought surgeons should keep themselves entirely independent of instrument makers' opinions. The duty of the surgeon is to amputate the limb or portion of it in order to get rid of disease or injury, and to save as much as possible of the limb. It does not matter from what position he gets the flap, provided it is one which will properly cover the bone. In connexion with his own amputations he entirely ignored instrument makers' opinions, and what Dr Cathcart had said showed that they could provide a useful artificial limb whether the stump was long or short. He thought Dr Cathcart had given too much prominence to one instrument maker in London, and he suggested that the name should be suppressed if the paper was published.

Dr Cathcart said he appreciated Professor Annandale's suggestion. He had the permission of the makers to use their names, and he proposed to give a reference to them, so that it might not seem that he had invented the answers they had given him. He was much interested in working at this subject. Strong assertions had been made by men who had professedly a great deal of experience, that they could do better for the patients if the surgeons took off more. It was right they should go thoroughly into these assertions, and he had come to the conclusion that they should save as much as they could. Still, it was satisfactory to know their reasons for it.

2. CASE OF ACUTE INTUSSUSCEPTION IN A CHILD THREE YEARS OF AGE SUCCESSFULLY RELIEVED BY ABDOMINAL SECTION.

By Thomas Annandale, F.R.C.S., Regius Professor of Clinical Surgery, University of Edinburgh.

I record a note of the following case in order to emphasize the importance—an importance which is now very generally acknowledged by surgeons—of early operation in cases of acute intussusception, or other forms of acute intestinal obstruction, when other means have failed to relieve the condition after a careful trial of them.

It is true that in a small number of cases of intussusception (about 6 per cent. in patients between the ages of 2 and 5 years, according to Leichtensterns[1]), spontaneous elimination by gangrene

[1] Treves upon Intestinal Obstruction.

of the gut takes place, but a certain proportion of such cases do not ultimately recover, but die from causes in connexion with the intestinal condition; and therefore it must be considered that, unless an acute intussusception is relieved in the early stages of the case, it is, especially in young children, a very fatal disease.

The treatment of this affection by enemata or insufflation can only be successful in its early stages, although a few exceptional cases have been recorded; and it should always be remembered that in the later stages this treatment is attended with considerable risks.

An interesting point in my case is the failure of the enemata, and the introduction of a bougie to relieve, although very gentle traction from within after the abdomen had been opened was sufficient to release the invagination. The success of this case is an additional proof of the value of traction upon intestines from within in certain cases of strangulated hernia, or some other forms of intestinal obstruction, as advocated by myself in a paper in the *Edinburgh Medical Journal* for 1873.

A further observation of importance in connexion with operative interference in cases of intussusception is, that reduction of the invagination is in the majority of instances more easily accomplished when the operation is performed during the early stages of the condition; and Mr Treves[1] has shown in his tables of statistics that the easier the reduction the less the mortality. When the reduction is easy, the mortality being 30 per cent.; and when difficult or impossible, 91·3 per cent. An additional advantage of an early operation is that, in the majority of cases, a limited abdominal incision, with a limited amount of interference with the abdominal contents, will be sufficient to relieve the condition.

Case.—J. G., a male child, æt. 3, was admitted into the Edinburgh Royal Sick Children's Hospital upon the 27th of September 1886, under the care of Dr Underhill, who has kindly given me the following notes of the history and progress of the case previous to my connexion with it.

Two days before admission to the hospital the child had suffered from pain in the abdomen with vomiting, which symptoms occurred quite suddenly and without any known cause. It was also noticed that several ounces of blood were passed per rectum, some of which was in the form of clots. When admitted the patient's general condition was good, and the abdomen was not much distended, and was not painful upon pressure; but upon the left side, towards the lumbar region, an elongated swelling about three inches in length and two in width was felt. This swelling, which much resembled in form a sausage, was not painful upon pressure, but it was very distinct.

[1] *Loc. cit.*

An examination by the rectum discovered a rounded mass, which could be pushed up by the finger, but at once descended when the finger was withdrawn. The condition was diagnosed to be one of intussusception, and the treatment suggested and at once carried out was small doses of opium internally, and an attempt, under the influence of chloroform, to push back the tumour in the rectum by the introduction of a bougie, so as to reduce the invagination. This having failed, enemata of warm water were tried, but without any good result. On the early morning of the 28th the patient still continued to vomit, and dark-coloured mucus was passed by the rectum. The intestinal obstruction being complete, and not having been relieved by the careful treatment referred to, I was asked, as consulting surgeon to the hospital, to see the patient, and Dr Underhill and myself were both of opinion that the time had come for operative interference as the only means of relieving the condition.

Accordingly, on the forenoon of the same day I opened the abdomen by a central incision about 2 inches in length. When the peritoneal cavity was opened into several coils of dilated small intestine first showed at the wound, and these having been pushed aside and the finger introduced, a pear-shaped tumour, about 4 inches long, was felt in the left lumbar region. This tumour was laid hold of with the finger and drawn towards the wound; but in doing so it somewhat suddenly dispersed, and about 8 inches of collapsed, wrinkled, and sodden-looking small intestine presented, and it was quite evident that the comparatively gentle traction exercised upon the small intestine had relieved the invagination and taken away the cause of obstruction. Immediately after the operation wind was passed freely by the rectum, proving that the obstruction had been relieved, and from this date the patient's progress was excellent. Upon the 30th the bowels were freely moved. On the 13th of October he suffered from severe abdominal pain, and his pulse became weak, and in the evening he had some vomiting; but a little simple medical treatment relieved the symptoms, and on the 15th he was quite well again. On the 17th he was able to leave his bed, and upon the 21st he was walking in the ward. He was discharged quite well upon the 28th of October.

Dr Bell said he was sure that the feelings of the members of the Society would be that the case was an admirable one. There could not have been a more precise and perfect example of what those cases ought to be. He thought that the treatment explained was the best and only mode. Sometimes, even in the most fortunate-looking cases, the result is not always so good. He was called upon to attend a little girl where the symptoms were precisely the same. He felt from the rectum the cæcal tumour, and opened the abdomen. It was easy to reduce the

small intestine without trouble or difficulty. Unfortunately the child died. It was under the care of an able man. There were no adhesions to any of the other parts of the intestinal canal. He thought he was right in saying that in many children who died from diarrhœa, three or four intussusceptions may be found on post-mortem examination, each apparently having formed in the later stages shortly before death, and probably such secondary intussusceptions may cause death in the fatal cases, even after operation has relieved the first one.

Dr Duncan joined in Dr Bell's observations upon the value of the record of the case mentioned by Professor Annandale. It belonged to that class in which the diagnosis was undoubted, and was therefore suitable for operation. Dr Duncan referred to a recent case where the post-mortem revealed peritonitis, there having been all the symptoms of sudden and complete obstruction. The diagnosis was exceedingly difficult in many of those cases. In children they had constantly a tumour at the rectum, and in the case referred to the intussusception took place lower down. The only case of intussusception in a very young child in which he performed laparotomy was one in which all other things had been tried without success. The case was exceedingly interesting in various respects,—first in regard to the fact that the intussusception had taken place of the small intestine into the large, and the piece of bowel had traversed the whole of the colon until it could be felt at the rectum. It was not very common to have not only one, but double intussusception into the cæcum. The interesting point in the case, however, was that he found the greatest possible assistance in reducing the bowel successfully by pressure as well as stretching. Steady pressure, such as is done in reducing hernia, was of great assistance in pulling the bowel out. The operation was a long and tedious one. The child died of shock within twelve hours. The mortality in these cases is always high.

Professor Annandale, in reply, thanked Mr Duncan for his remarks, and again emphasized the fact that cases of acute intussusception in children were most fatal. When an early operation is performed, the reduction is comparatively easy, and he expressed his opinion about the importance of the early operation. Of course one must expect in all forms of intestinal obstruction to meet with a complication. He had seen the other day an interesting case of a woman in the last stage of intestinal obstruction. She had been examined previously by several of his colleagues, who were of opinion that there was some obstruction in the small intestine. Under chloroform, a tight malignant stricture of the rectum was discovered. The sigmoid flexure of the colon was then exposed with the view of performing inguinal colotomy, but as this gut was found collapsed, a dilated portion of the small intestine was seized and stitched to the wound so as to make an artificial anus above the obstruction. The patient died two

days after, and at the post-mortem a very small strangulated obdurator hernia was found. The portion of intestine which protruded and was strangulated did not include the whole circumference of the bowel, and was scarcely a quarter of an inch in length.

Meeting VI.—February 20, 1889.

Dr John Smith, *President, in the Chair.*

The President said,—Gentlemen, you are aware that the Special Meeting of the Society this evening is called for the consideration of a subject of very great and wide-spread interest and importance, both in a medical and a legal point of view. Many difficulties beset the medico-legal aspect of the subject, and our object now is not to bring forward individual cases, but rather to discuss the general question, and to aid in the elucidation of those points, of which there are a considerable number, requiring mature deliberation, in order to their being safely and judiciously dealt with in any legislative enactment which may be necessary upon this subject. The matter, you are aware, has been brought before the Secretary for Scotland, and a Bill in connexion with it has been framed by Mr Charles Morton, W.S., the late Crown Agent for Scotland. The difficulties of which I speak, and which will be brought more prominently before you this evening, are the considerations and precautions which are required in framing any measures with the view of being adopted for the purposes of the Bill which is proposed to be brought before Parliament. Such considerations will be recognised as those attaching to the admission of persons either as voluntary inmates or by committal as compulsory patients. There are responsibilities here which must be taken into account. The next thing is the detention of such persons, whether voluntary or by committal; to consider what powers are to be conferred for the detention of these patients, and in whose hand these powers are to be vested. Again, we must take into consideration the exact definition of what constitutes loss of self control, of what constitutes inability to manage one's affairs, or even of danger to self or others; and perhaps it might be well to consider whether any modifications are required in these cases where the inebriate is a habitual and continuous drunkard, or one of an occasional nature, where, perhaps, the lapse of months without any mental aberration whatever takes place between the outbreaks, which, however, may be serious at the time.

Before calling upon Prof. Stewart to open the discussion, I wish the Secretary to intimate the apologies he has received

from several gentlemen who are unable to be present this evening, and I should like him to read at length some of them which are of considerable weight, such as Professor Sir Douglas Maclagan's.

The Secretary (Dr James Ritchie) intimated apologies from the following:—Professor Sir Douglas Maclagan, Professor Gairdner of Glasgow, Professor Muirhead, Professor Kirkpatrick, Dr Morton, President of the Faculty of Physicians and Surgeons, Glasgow; The Solicitor General, Sheriff Ivory, Sheriff Æneas Mackay, Sheriff Guthrie Smith, The Dean of Faculty, Mr Charles Morton, Sir Charles Pearson, Messrs Arthur Alison, J. Henderson Begg, T. D. Brodie, Charles Scott Dickson, Charles J. Guthrie, T. G. Murray, Drs Alfred Daniell, Angus Fraser, Aberdeen; Samuel Moore, and Alex. Robertson, Glasgow.

He stated that although several of these gentlemen had expressed themselves very strongly in favour of further legislation for the care of habitual drunkards, he would read only three of their letters.

Professor Sir Douglas Maclagan wrote:—"I cannot now go into details, but I beg to express my cordial approval of the principles of Mr Charles Morton's Restorative Homes (Scotland) Bill. To do any good we require to have complete power, under proper legal restraints, of saving these wretched people from themselves, and it appears to me that the legal requirements are ample for securing any one against an infringement of that which is, as regards this subject, an intense humbug—the liberty of the subject. I really have some difficulty in seeing who can be injured by this Bill. It cannot be the victim, him or herself, whom we wish to save from ruin—soul, body, and estate. It cannot be his or her relatives, whom we wish to rescue from worry and misery. It cannot be our excellent asylums, both chartered and private, of which in Scotland we have so much reason to be proud, because as the law stands we cannot legally commit the habitual drunkard to their custody. Why, then, should not Parliament give us a chance and enable us to show, as I am sure we would do, that with proper but safe guarded authority we could save these sad victims of that which we all recognise as a form of disease. I hope that the opinion of the Society will be in favour of extended powers, and that the Bill, avowedly tentative, will be confined to Scotland."

Mr J. B. Balfour, Q.C., wrote:—"The subject is one the interest and importance of which cannot be over-estimated. I should be very glad if anything could be done in the way of legislation to mitigate so great an evil."

Professor Gairdner, Glasgow, wrote:—"I am strongly persuaded of the absolute necessity of a change in the law with respect to habitual drunkards."

Prof. Grainger Stewart said,—The subject which we are to discuss this evening belongs in a somewhat special way to this Society. It is more than thirty years since Dr Peddie formulated for the profession and for the public, in a very able paper read here, his views as to the necessity for legislative enactments for the benefit of certain classes of inebriates and their families and estates. In the discussion which followed, Sir Robert Christison, Mr Murray Dunlop, M.P., Prof. Laycock, and others, took part; and from that time, as well as in some measure before it, the members have been interested in the question. In the opinion of the Council, the present is an appropriate time for renewing the discussion; for we have now had ten years' experience of the working of "The Habitual Drunkards Act, 1879," and by the light of this experience we are in a position to reconsider the question. Further, many members of the medical and legal professions in Edinburgh have had the opportunity of studying the provisions of the Bill, which has been prepared by a very able and distinguished lawyer, who was for a number of years, and under several administrations, Crown Agent for Scotland, in which it is proposed to deal with the question on new lines. This suggested Bill is the more deserving of study, as it is known that Government officials have been carefully considering its proposals, and the opinion has been expressed in influential quarters that Mr Morton's Bill would soon be introduced into the House of Commons, for those in authority had expressed great sympathy with it, and their desire to further it. For these reasons, and because I hope that there is now a pretty general consensus of opinion in favour of something further of a legislative nature being attempted, I have willingly complied with the request of the Council that I should open the discussion this evening.

I wish that it had been possible to have had it brought forward by Dr Peddie, who has done more to draw attention to this subject than any other man in this country or abroad, or by Sir Douglas Maclagan or Sir Arthur Mitchell, whose large and special experience would have fitted them so well to bring before us the state of the question. I am glad that although the duty has fallen upon me, the Secretary has read us the important expression of opinion of Sir Douglas Maclagan, so that he may be said to have taken the first word in our discussion; and we have the benefit of the presence of Dr Peddie, and of others eminently well qualified to speak, and may hope to hear their matured opinions this evening.

The difficulties of dealing with the topic are two, viz.,—first, that the necessity for something being done is so obvious as scarcely to admit of argument or illustration; and, second, that the devising of suitable plans is beset on every side with embarrassing considerations.

The experience of every medical practitioner, of every family lawyer, and of too many of the general public, supplies ex-

amples of intemperance of many kinds. There is the steady tippler, who takes small quantities of alcohol at intervals during the day, never getting intoxicated, and yet certainly shortening his life by such indulgence. There is the man who gets drunk almost every night, but keeps perfectly steady and attends well to his work during the day. There is the convivial victim of intemperance, who is sure to exceed on every festive occasion. There is the ordinary case of delirium tremens or acute alcoholism. There is the victim of chronic alcoholism and of alcoholic paralysis. There is, besides, the maniac, the monomaniac of suspicion, the melancholic, and the general paralytic, each of whom may owe his insanity to his drunken habits. But besides all these there is yet another class, and it is for them that legislation is specially required. Sometimes such patients look wonderfully well in the intervals between their attacks. Sometimes they show all the features of the chronic alcoholic. The face is flabby, sometimes pale, sometimes with red or coppery nose or cheeks; the muscles are jerking and unsteady, the tongue is tremulous and furred, the throat is congested, and the breath smells of more or less altered liquor. The stomach is frequently disordered; the appetite poor, especially of a morning, with a tendency to sickness, with vomiting of mucus; the bowels are irregular; the liver is extended beyond its normal limits, congested, and rather tender on pressure. The heart acts with little vigour, often too quickly, and sometimes with fits of irregularity or palpitation. There is a frequent tendency to clear the throat, and some cough. The skin is soft, and tends to perspire; the urine is copious, sometimes pale and of low sp. gr., sometimes clouded with urates, or depositing uric acid, or showing some albumen. But the nervous system is chiefly changed. There is undue sensitiveness to impressions, jerky and unsteady muscular movement, with incapacity for sustained exertion. There is a liability to sudden flushings or pallor, excessive dryness or excessive perspiration of skin. There is sleeplessness, nervous irritability, loss of the faculty of concentration, and impairment of memory; while, perhaps, from time to time after a drinking bout there is a regular attack of delirium tremens or epileptiform convulsions, or of alcoholic paralysis, or of mania *a potu*. But at all events there is an insatiable craving for drink, sometimes constant, sometimes coming on occasionally,—a craving which is declared to be absolutely irresistible; not that the drinking necessarily gives pleasure, but the desire cannot be resisted. No end of cunning and ingenuity is manifested in the attempt to get supplies of liquor. The moral nature becomes so debased as to be absolutely incapable of distinguishing truth from falsehood; the most ingenious tricks are resorted to, the most unblushing lies are told. The patients lie with a calm resoluteness, assure one with a pleasant smile that they have never in any degree exceeded; and they are never put out of countenance if you draw the half emptied bottle from

beneath their pillow, or otherwise convict them of the most flagrant untruth. They are liberal of promises in the highest degree, are willing to acquiesce in any opinion which one may express to them. Instead of the sweet reasonableness which a recent much-lamented writer used to describe as one of the best qualities of man, they have an unreasonable sweetness, so far as words and promises go. Their manner is often tinged with a peculiar sadness. They seem to contemplate their own careers with a kind of melancholy complacency. But while such moods of mind are common, a great change manifests itself during the drinking bout, or when they are seeking for the gratification of their appetite. Then they frequently get into trouble; they steal or otherwise bring themselves within the grasp of the law; and some patients of this kind are constantly getting imprisoned for longer or shorter periods. Mr Smith, the governor of the prison at Ripon, gave in his evidence before the House of Commons in 1872 some statistics regarding such a case, whose history was known to him during a period of 25 years. The subject, who was a woman, had been 17 times in Wakefield jail for periods of from 3 days to 3 weeks, 11 times in Leeds jail, 15 times in Northallerton jail, 15 times in Ripon jail, all for being drunk and disorderly. She was thus imprisoned 58 times; and of the 25 years of which I have spoken, spent 5 years 9 months and 20 days in prison. Dr Peddie told the same Committee of the wife of a respectable tradesman who had for 42 years been a habitual drunkard. Every possible means had been tried for her cure, but without avail. She had been boarded in different parts of the country, prevented access to drink, shut up on different occasions in a lunatic asylum, 15 times in different places of shelter and refuge, 15 times convicted of drunkenness and disorderly conduct, and sentenced to various terms of imprisonment, running from 14 to 60 days, and her periods of imprisonment had amounted in all to 778 days, besides 200 nights spent in police cells. He told also of a son of this woman who died in prison at the age of 38. He began to drink when a mere lad, and although quiet and amiable when sober, had at times an irresistible impulse to drink, and then became furious and dangerous, and much given to thieving. Thirteen times he was convicted of being drunk and disorderly, 4 times for theft; he spent 922 days in prison, besides many nights in the lock-up when he was found in the streets drunk and incapable. The great special characteristic, then, of this form of intemperance is, that the victim is possessed of an irresistible and insatiable craving for liquor; it may be constantly, it may be in paroxysms recurring at longer or shorter intervals. This craving must be gratified at any cost; the victim becomes, as Dr Peddie has said, regardless of honour or truth, unaffected by appeals to reason or self-interest, by the tears of affection, or by the suggestion of duty either to God or man.

A Committee of the House of Commons defined the class as including those who, notwithstanding the plainest considerations of health, interest, and duty, are given over to habits of intemperance which render them unable to control themselves and incapable of managing their own affairs, or such as to render them dangerous to themselves or others.

Now experience has made it abundantly plain that little or no benefit accrues from punitive confinement in jails, or from short periods of residence in asylums for the insane, or in houses of refuge or shelter, while reason and experience both lead us to believe that confinement in suitable homes for lengthened periods might, in a certain proportion of instances, effect a cure, especially if the treatment were applied at a comparatively early period of the disease. The results of experience have been distinctly encouraging. I am unable to attach much importance to the returns published in regard to some of the minor institutions in England, as their results surpass what we are entitled to expect. The Government report for the year 1887 states, on the authority of the manager, that two-thirds of the patients discharged from the Westgate-on-Sea Retreat were permanently cured; also that most of those who remained in Walsall Retreat for twelve months did well; while the Hales-Owen Retreat showed a fair proportion really cured, but results would have been better if patients had placed themselves earlier under restraint, and remained at least twelve months. The Twickenham results are described as more than encouraging. But the returns of the Dalrymple Home, in which the utmost confidence is placed by those well fitted to judge, show that during three and a half years after the opening of that institution there were 103 admissions and 85 discharges; the average period of restraint being six and a half months. Of the 85 discharged, 36 are reported doing well, 2 are reported improved, 27 not improved, 1 insane, 3 dead, 16 not heard from. I could adduce many striking results from some of the American institutions, but shall ask you to fix your attention upon the facts which I believe to be reliable and carefully sifted, that of the 103 admissions to the Dalrymple Home, 36, or upwards of one-third, are reported as discharged and doing well. Such a result is unmistakably encouraging.

Now let us see what legal remedies have been proposed to meet these evils. No one has proposed to legislate for the mere tippler, for the man who occasionally gets drunk upon convivial occasions, nor even for the man who, like a patient whom Dr Skae described to the Committee of the House of Commons, is carried drunk to bed every night, but who is quite able to do his work during the day. There is no need for legislation in regard to delirium tremens or chronic alcoholism. The Lunacy Laws provide with ample distinctness for the cases of mania *a potu* and the other varieties of insanity. It is the special form which lies upon the borderland

between drunkenness the vice and obvious madness for which it is believed we might legislate with success. I have no wish to involve myself in questions as to definition of insanity generally, and of the precise relationships between it and intemperance, or to attempt to formulate such a definition of the disease we are now considering as would satisfy in every theoretical detail a specialist or a lawyer. I appeal to those who are experienced practitioners for confirmation of the statement, that many of the cases to which I am referring are capable of easy and definite recognition, and that for practical purposes the definition which I have already quoted from the Report of the Committee of the House of Commons is quite sufficient. If this be accepted, what scheme of legislative enactment might be expected to prove useful? Dr Peddie, in the Appendix to his first paper, gave a series of nine suggestions, which I shall summarize:—

1. That four establishments, not lunatic asylums, should be opened in Scotland for the reception, comfort, and cure of dipsomaniacs.

2. That a board, consisting of a magistrate, justice of the peace, a clergyman, and a physician, should meet from time to time to consider cases, grant orders for reception and discharge, make regular visits to the establishment in order to see that the various arrangements for the care, comfort, and cure of the inmates are properly carried out, and in general to consider all matters connected with the proper working of the scheme.

3. That appeal should be to the Lord Advocate or Lunacy Commission.

4. That applications for protection and cure might be made voluntarily by the dipsomaniac himself, he undertaking to submit to the rules of the institution and remain as long as the directors think it necessary. Compulsory restraint might be applied for by any friend, relative, member of the community, or parochial board, or the procurator-fiscal for the public interest.

5. Applications for compulsory restraint should state in the petition to the sheriff the grounds on which they are made.

6. That they must be attested by witnesses and by the medical attendant of the individual. The sheriff should also require an opinion from another medical practitioner appointed by himself, should then transmit the evidence in writing to the board of direction for the district.

7. The board, being satisfied, should notify their opinion to the sheriff, that he may grant warrant, and they make arrangement for the admission to the establishment.

8. That no warrant or certificate should be granted for a shorter period than six months or longer than two years; but that the friends might remove the patient under certain restrictions in a shorter time, or that the detention might, under certain conditions, be prolonged.

9. That no individual restrained under the regulations should be considered as altogether deprived of civil rights.

But these proposals never came under the consideration of Parliament.

In one of the Lunacy Acts provision is made for the admission of inebriates into lunatic asylums, they consenting to enter and submit to treatment for a certain time. This provision is, in my opinion, an important and valuable one. I have in many instances urged this step upon the habitually intemperate, and have sometimes induced them to take advantage of the Statute. I am glad to say that I have seen complete and apparently permanent cures effected in such cases, and I can never be otherwise than grateful to the officers of various institutions who have been the means of rescuing individuals from this otherwise hopeless condition. Still, I must admit that I have found it difficult to avail myself of this provision of the Act, for the patient himself and his family often shrink from the idea of confinement in a lunatic asylum, and from the consequences that might result to the subsequent position of himself and his family, and indeed it must be admitted that the arrangement is otherwise unsatisfactory in respect that it is not good for the habitual drinker to be placed in the society of ordinary lunatics, and because patients of this class often prove troublesome and unsatisfactory inmates of asylums, interfering with the working of the institution, tampering with attendants, and stirring up the ordinary patients to discontent and complaint.

In 1870 the late Dr Donald Dalrymple, M.P. for Bath, introduced a Bill in the House of Commons providing for the admission of habitual drunkards into retreats. This Bill provided for the establishment of inebriate reformatories, sanctuaries, or refuges, and for the maintenance of habitual drunkards therein to be charged on the rates; for the establishment by Boards of Guardians of a special place for the treatment of habitual drunkards; for the committal of a pauper habitual drunkard to a retreat, on the production of two medical certificates, for a limited period; and for the committal, without certificate, of any person committed for drunkenness three times within six months. Admission might be voluntary or compulsory on the request of a near relation, friend, or guardian, or on the certificate of two duly qualified medical practitioners and the affidavit or declaration of some credible witness. This Bill did not pass; but in 1872 a Committee of the House was appointed to inquire into the best plan for the control and cure of habitual drunkards, and they reported "that there is entire concurrence of all the witnesses in the absolute inadequacy of existing laws to check drunkenness, whether casual or constant, rendering it desirable that fresh legislation on the subject should take place, and that the laws should be made more simple, uniform, and stringent. That occasional drunkenness may, and very frequently does, become confirmed and habitual, and soon

passes into the condition of a disease uncontrollable by the individual, unless, indeed, some extraneous influence, either punitive or curative, is brought into play. That self-control is suspended or annihilated; moral obligations are disregarded; the decencies of private and the duties of public life are alike set at nought; and individuals obey only an overwhelming craving for stimulants, to which everything is sacrificed. That this is confined to no class, condition, or sex, and hardly to any age. That it is in evidence that there is a very large amount of drunkenness among all classes in both sexes which never becomes public, ... which is probably even a more fertile source of misery, poverty, and degradation than that which comes before the Police Courts: *for this no legal remedy exists, and without further legislation it must go on unchecked.* Legislation in such cases was strongly advocated by all the witnesses before the Committee. That the absence of all power to check the downward course of a drunkard, and the urgent necessity of providing it, has been dwelt upon by nearly every witness, and the legal control of an habitual inebriate, either in a reformatory or in a private dwelling, is recommended, in the belief that many cases of death resulting from intoxication, including suicides and homicides, may thus be prevented. That this power is obtained easily at moderate cost, and free from the danger of abuse and undue infringement of personal liberty, has been stated in evidence by quotations from American and Canadian statutes, as well as by the witnesses from America. That it is in evidence, as well from those who have conducted and are still conducting reformatories for inebriates in Great Britain, as by those who are managers of similar institutions in America, that sanatoria or inebriate reformatories are producing considerable good in effecting amendment and cures in those who have been treated in them. That the proportion of cures is not larger is attributed by all the witnesses to a lack of power to *induce or to compel the patient to submit to treatment for a longer period—and that power is asked for by every one who has had or still has charge of these institutions.* Without such a power it appears that the results must be imperfect, disappointing, and inadequate to the efforts made."

The Committee therefore recommended that—"Sanatoria or reformatories for those who, notwithstanding the plainest considerations of health, interest, and duty, are given over to habits of intemperance, so as to render them unable to control themselves, and incapable of managing their own affairs, or such as to render them in any way dangerous to themselves or others, should be provided."

The Committee further recommended that these sanatoria should be divided into two classes—"(A.) for those who are able out of their own resources, or out of those of their relations, to pay for the cost of their residence therein; these, whether promoted by private enterprise or by associations can be profitably and success-

fully conducted;" and "(B.) for those who are unable to contribute or only partially, sanatoria must be established by State or Local Authorities, and at first at their cost, though there is good reason to believe that they can be made wholly or partially self-supporting."

The Report then proceeds as follows:—"The admission to these institutions should be either voluntarily or by committal. *In either case the persons entering should not be allowed to leave except under conditions to be laid down, and the power to prevent their leaving should be by law conferred on the manager.* Though practically this power would be seldom put in force, *it would be useless to establish these institutions without it.*

"The patients under Class A. should be admitted either by their own act or on the application of their friends or relatives under proper legal restriction, or by the decision of a local court of inquiry, established under proper safeguards, before which, on the application of a near relative or guardian, or a parish or other local authority, or other authorized persons, proof shall be given that the party cited is unable to control himself, and incapable of managing his affairs, or that his habits are such as to render him dangerous to himself or others; that this arises from the abuse of alcoholic drinks or sedatives, and he is therefore to be deemed an habitual drunkard.

"The period of detention should be fixed by the court of inquiry or by the magistrates, but may be curtailed upon sufficient proof being given that the cure of the patient has taken place."

No legislation was accomplished in the direction of the Committee's Report for seven years.

In 1877 Dr Cameron, M.P. for Glasgow, brought in a Bill similar to that of the late member for Bath, but leaving it to a jury instead of a magistrate to decide whether an individual for whose compulsory committal to a retreat application was made was an habitual drunkard or no; and at last, in 1879, after much opposition, "The Habitual Drunkards Act" was passed, Dr Cameron having succeeded in piloting it through the House of Commons, and Lord Shaftesbury through the House of Lords. This Act defines a habitual drunkard as a person who not being amenable to any jurisdiction in lunacy, is notwithstanding, by reason of habitual drinking of intoxicating liquor, at times dangerous to himself, herself, or others, or is incapable of managing himself or herself, and his or her affairs. It provides for voluntary admission only, and requires appearance before two justices of the peace, with other precautions. There are many details of precautions, into which it is not necessary for me to go.

This Act was at first passed for a period of ten years, but in 1888 a short Bill was introduced and passed into law rendering this legislation permanent, and requiring appearance before only one justice of the peace instead of two. The main deficiencies of

this Act are that the rules for obtaining admission, as at present arranged, are not sufficiently simple; that there is no power of compulsory confinement; that a period of restraint sufficiently prolonged for the purpose cannot be obtained, and probably that the restraints are not sufficiently complete. Such is the present state of legislation on the matter.

Mr Morton's Bill is framed on the lines recommended by the Select Committee of 1872, but proposes to deal only with the case of persons who are able to pay a moderate board. He proposes it as a tentative measure applicable to Scotland, and fitted to gain experience to guide in the establishment of similar institutions of the pauper class, and in other parts of the Empire. In its preamble he states that—"Whereas by the Acts 20 & 21 Victoria, c. 71, 25 & 26 Victoria, c. 54, 29 & 30 Victoria, c. 51, and 34 & 35 Victoria, c. 55, provision is made for the care and treatment of lunatics in Scotland, but no adequate provision is made in these Acts, or by the law of Scotland, for the care and proper treatment of persons who, although not exhibiting such symptoms as would warrant a medical practitioner to grant a certificate for their confinement in a lunatic asylum, are yet labouring under a special form of mental disorder, the chief distinguishing features of which are—excessive and secret indulgence in intoxicants, the craving for which is more or less persistent, or occurring in fits, with remissions at intervals of time, and a marked change in the mental powers and moral character. And whereas such persons, by their habits and conduct, embitter, disturb, or break up domestic or social relations, and in many cases bring themselves, or families, or others into a state of degradation, or ruin, or danger of life, it is expedient and necessary for the protection both of them and others that such persons as above described should be cared for, by providing means for placing them in temporary retirement, in a place of residence other than a lunatic asylum, under proper care and medical treatment, and under such restraint as will prevent them from having opportunities of continuing such vicious and ruinous indulgences, whereby a permanent cure may reasonably be expected." In this preamble it will be observed that there is no reference to other kinds of indulgence than the alcoholic—none to morphia or chloral; and it may be a question whether this ought not to be considered. The memorandum prefixed to the Bill discusses its provisions with great care, and explains many of the proposals. Recognising the necessity for a qualified and central authority to carry out its provisions, the Bill ordains that the Board of Commissioners in Lunacy, and the District Lunacy Boards, should be entrusted with this duty, and that the small assessments which may be required should be raised along with those for the purposes of the Lunacy Acts. It proposes to give the Lunacy Board power, after due inquiry as to the necessity for the establishment of homes in each district for the recep-

tion of inebriates, to provide general accommodation in a district home, or to license the establishment of private homes for care and treatment under due medical supervision. The Commissioners in Lunacy are also authorized to frame rules both for the conduct of district and private homes, and scales of charges for patients of different grades as respects their ability to pay for board and treatment,—all this subject to the consideration and approval of the Secretary for Scotland.

It proposes that patients should be admitted to one of the licensed homes upon his own application to the superintendent. If he should refuse to apply, the Bill gives power for admission and forcible detention by the following process:—Any member of the patient's family, any other near relative or friend taking interest in him, or when there is no relative or friend to act, a magistrate in the public interest, may present an application to the sheriff to grant an order for reception and detention in a home.

It is not proposed that the application should be intimated to the patient, but the applicant must make a solemn statutory declaration equivalent to an affidavit, fully setting forth the circumstances of the case. Two private friends of the inebriate, who are well acquainted with him, his family, and circumstances, must make similar solemn statutory declarations; that one, or if thought proper, two medical men should also certify, upon soul and conscience, as to the patient's condition. Upon such evidence the sheriff is to proceed to consider whether he ought to grant an order for reception and detention. It is believed that in this way the necessary powers are given, without risk of interference with the liberty of the subject.

With regard to the period of detention, it is proposed, in accordance with the recommendation of the Select Committee of the House of Commons and Upper House, that it should be for twelve months at least; but power is granted for earlier discharge should circumstances require it, or for prolonged detention if that course should be found necessary.

It is provided that the patient may at any time appeal to the sheriff for recall of the order and discharge, or to the Commissioners in Lunacy, with, of course, the right of appeal to the Secretary of State.

It appears to me that what has been said makes it clear that an urgent need exists for legislation, if the proper legislation can be devised; that the existing legislation, although to a certain extent of value, is insufficient to meet our necessities; and that the general scope of Mr Morton's bill is excellent, and many of the details admirably devised, so as to give us something distinctly in advance of what we have attained. The safeguards provided for the liberty of the subject are amply sufficient, both as to the precautions taken to prevent wrongous admission, and those to diminish the risk of undue detention. Although it may seem

somewhat hard to add to the duties already discharged by the Lunacy Board, certainly no existing institution could compare with it in fitness for the work, and it would be difficult to conceive how a board could be devised better fitted to discharge the duty, even if such a board were to be framed of set purpose. It may be held by some that there would be no need of establishing district homes at the expense of the rates, inasmuch as private establishments of the kind would be speedily set up if legislation of a permanent kind warranted their formation; and it is possible that this view is correct. But in its main points it appears to me that if such an enactment as this Bill proposes was passed into law, we might reasonably count upon a perceptible diminution of the sum of human misery, the cure of not a few who have become the subject of this evil, and much benefit to their relatives and estates.

Dr Yellowlees, Gartnavel Royal Asylum, said—I certainly would not have willingly intruded so soon. I would rather have listened, but since you have asked me, and since trains are inexorable, I will say a few words now. I think Prof. Stewart has done a great deal in clearing the ground. Nothing more need be said about the characteristics of the habitual drunkard. He has put them so admirably before us, and so perfectly are they sketched in the preamble of the Bill, and also in the Report of the Dalrymple Commission, I need say no more of them. We all know these cases quite well, and we know from our own observation harrowing details of the danger, the misery, and the ruin that such a patient entails upon himself and his family. I need say nothing more as to that. I take it that the chief object of our meeting is not to discuss this aspect of the question, but to let the public know how strong and how unanimous our feeling is that such cases demand far greater care than we can at present give them. At least we desire to inform such of the public as do not already know. There are no sceptics among those who have had in their own family or acquaintance a habitual drunkard. That is the saddest argument and the most convincing one. We are all agreed as to the misery and distress thus caused, and as to the necessity for something being done, and we are all agreed as to the helplessness of such cases without some one helping them. The misery of it is that most of these people will not have the help. We are all agreed, too, as to the frequent hopelessness of cure. He was a very sanguine man who found two-thirds of the cases in one of the retreats recover! I am quite sure that those of us who have the widest experience of such cases have the darkest tales of failure to record. I think that the only hope is in enforced abstinence, and the abstinence is useless unless continued for a long time—a year at least. We are all agreed, too, that our present mode of dealing with such cases is a miserable

failure. If poor people, they get into jail. That is not to be regretted. It is the best thing that could happen to them, as the law now stands. Referring to the case mentioned by Dr Stewart, I do not think it is to be regretted that that man was five years in jail. It was best for the man and best for the public, and the public have a right to be considered; if they could have kept him longer, it would have been far better. The futility of the jail treatment for short periods is perfectly certain, but there are some "habituals" so bad and so hopeless that the only course is to put them beyond doing mischief to their neighbours. If the patient is not poor, you may try to get him into an asylum as a voluntary patient—that is, provided you can get an asylum superintendent good natured enough to admit him. I habitually and deliberately refuse such patients. I refuse them for their own sakes, because they presently get so absolutely certain that they are well that you cannot persuade them to remain long enough to get any real good, and I object to them for the sake of the other patients as well. I say you have no right to impose the company of such liars and mischiefmakers upon respectable lunatics. The next thing you probably do, if you cannot get them into an asylum, is practically to banish them. You send them to a remote part of the country, to Skye or Orkney, if you can get people to keep them—where you deprive them of money, and where they associate with people as bad as themselves. I have often thought that the moral tone of these inebriate refuges must be of the lowest. And still another most miserable recourse is to send them abroad, to let them drink themselves to death where they wont disgrace their friends. The present modes are thus miserable failures. Legislation hitherto has been useless, and the Habitual Drunkards Act a complete failure. This was fully expected at the time. Dr Cameron, who fought hard for the Act, told me that it was hopeless to try to carry the compulsory clause. The choice was between this Act or nothing, and he took this with the hope that something better might be got next time. We are all satisfied as to the need of something better, and this seems a good time to legislate, as the Habitual Drunkards Act expires next year (I am just told it has been renewed again); but I doubt very much if you will get anything so sensible and so needful carried through, as the time of Parliament seems to be taken up with discussing such very important questions as what kind of breeches an Irish patriot shall wear. Mr Morton's Bill is only too good. It is too good, because it is a great deal more than is attainable. I wish it could be got, but I have no hope whatever that many of its provisions will be carried out. I have no hope that the Commissioners in Lunacy will undertake the care of habitual drunkards in addition to their present duties, though none could care for them so well; and I have no hope whatever that the assessment clauses which the Bill contains could be

carried. If they are dropped, then the only practical difference between Mr Morton's Bill and the Habitual Drunkards Act is the compulsory clause. At present, without that compulsory clause the Habitual Drunkards Act is useless—sadly useless.

Dr Peddie said,—I am glad that our Council has brought up this subject for discussion, seeing that Mr Morton's draft Restorative Home Bill is at present in the hands of the Secretary for Scotland; and it is very satisfactory that Dr Stewart has at their request undertaken to explain the scope of that Bill, and those delicate and important points which are concerned in the question. I hope that this meeting of the Society may be as unanimous in opinion on the question as was the case thirty-one years ago, when I brought the matter before it, and read a paper on the subject. That paper when published was circulated extensively; and the suggestions then made, following as they did the recommendation three years previously by the Scottish Lunacy Commission of 1855 in their Report of 1857, for "prolonged detention in asylums of cases of insanity arising from the habit of intemperance," created a widespread interest, and was most favourably commented on at the time in almost every newspaper and journal in the country.

Between that time and the passing of the Habitual Drunkards Act twenty-one years elapsed. Dr Stewart has given something of a sketch of the fits and starts of the agitation on the question during that period of time; and the outcome of all the agitation was the present Act. The Bill then brought in—it was not the first Bill—by Dr Cameron, was a strong measure on the subject, but it came out of Parliament an emasculated one, a very feeble Act, which has done very little, but certainly some good, as it has advanced the question somewhat; but now we have before us a draft Bill which, if carried, will, I believe, accomplish a great deal more, although it certainly cannot altogether meet the grievous evil which exists in our midst. There is no wonder that the Act of 1879 is disappointing, because it dropped the essential clause for good, namely, the compulsory clause. The voluntary clause was likewise almost valueless by being hedged with obnoxious and debarring requirements. There was also dropped from the proposed measure another provision, viz., that clause for extending the Act beyond the upper and middle classes. Any provisions made should be available for those of the labouring, pauper, and criminal drunkards as well. The preamble of the Bill now before us has been read; and so far as I am able to give an opinion, having given a good deal of attention to the matter during past years, it is, I believe, judiciously constructed, and I do not know that we could get a better Act than what may be founded upon it. It has been drawn upon the lines of the Report of the Select Committee of the House of Commons in 1872, and signifies what seems most desirable in the case. In

complying, therefore, Mr President, with the request of your Council to follow the lead of Dr Grainger Stewart in introducing the subject for this evening's discussion, I hope I may not tread too much on the ground gone over by him while emphasizing in some measure what has been said on some of the most important features of the draft Bill. This I have attempted to note under three heads, but will add as a fourth what I consider to be a defect in the proposed Act.

1. The assertion in the Bill that it has to deal with a special form of mental disorder; and that its provisions proceed on the supposition of the probability that cure or alleviation may frequently be effected.

2. That any arrangements for the establishment of "restorative homes," unless providing for easy voluntary admission to such, and, if need be, for compulsory enforcement and power to detain, must cripple and seriously nullify legislation designed for personal and relative benefit.

3. That the safeguards afforded by this Bill are amply sufficient for the protection of the liberty of the subject, and all interests connected with individuals, families, and the public.

4. That the defect of the proposed Act is in its limited application to the well-to-do classes, and in not extending its provisions to the labouring, the pauper, and the criminal classes.

1. The assertion in the Bill that it has to deal with a special form of mental disorder; and that its provisions proceed on the supposition of the probability that cure or alleviation may frequently be effected.

The right understanding that a morbid mental condition exists in those individuals for whom legislation is sought, lies at the bottom of the whole question, a condition which requires mixed physical, mental, moral, and religious treatment in a *home* or a *retreat*, as if they were *patients* in an hospital, but not in an asylum, if that can be avoided.

It is again and again asked by those who are opposed to legislation in the case of habitual drunkards, How are you to draw the line between drinking the vice and drinking the disease, and consequently carry out a just administration of law as regards control? But the diagnosis in individual cases must be perfectly easy to common-sense observers; indeed, it should be more easy than in the general run of insane cases, or sometimes of medical disease; for not only will an opinion be formed from physical manifestations which are sufficiently marked, but substantiated and confirmed as they must always be by what is seen in the conduct of each person, and from the testimony of reliable witnesses as to existing circumstances and statements of historical fact. The preamble of the proposed Bill read by Dr Stewart is most admirable as to the points and limitations for which an Act is

designed, both in the way of definition and description, and could scarcely, I think, be improved.

The habitual drunkard is not the ordinary social drinker—one who imbibes freely even to intoxication at public feasts or at markets, or with boon companions, or who soaks a great deal daily, or resorts to frequent "nips" for the love of the drink, while yet tolerably fit to discharge the ordinary duties of life. But he is one whose desire has originated as a disease, or has passed from intemperance into a condition in which there is an irresistible, ungovernable, uncontrollable craving for intoxicants which he gives way to solitarily, stealthily, and deceitfully; and who is notoriously untruthful as to the desire and its indulgence, and utterly regardless of consequences to himself or others, even in spite of the most sacred social and moral obligations.

Examples of all these characteristics I could easily supply from personal experience did your time permit; but I am sure that all present of much experience in practice must be able to recall instances in corroboration of what I have stated. I shall only quote one short passage from the evidence I gave before the Select Committee of the House of Commons in 1872 as a sample of what I have often met with (*Report*, p. 49, answer to question 939, twelfth line from top)—"I never yet saw truth in relation to drink got out of one who was a dipsomaniac; he has sufficient reason left to tell these untruths and to understand his position, because people in that condition are seldom dead drunk: they are seldom in the condition of total stupidity; they have generally an eye to their own affairs, and that is the main business of their existence, namely, how to obtain drink. Then they will resort to the most ingenious, mean, and degrading contrivances and practices to procure and conceal liquor, and all this, too, while closely watched, and succeed in deception, although almost fabulous quantities are daily swallowed. In many of those cases with which I have had to do, ladies as well as gentlemen—and the former are generally the worst so far as untruthfulness and ingenuity are concerned—I have had the most solemn asseverations that not a drop of liquor had crossed their lips for many hours, when they could not have walked across the floor; that not a drop of liquor was within their power, when I would find bottles of liquor wrapped up in stockings and other articles of clothing, concealed in trunks and wardrobes, put up the chimneys and under beds or between mattresses; and on a late occasion, in the case of a lady, after all means had failed in discovering where the drink came from, on making a strict personal examination found a bottle of brandy concealed in the armpit, hung round the neck with an elastic cord, so that she might help herself as she pleased. The next morning, on seeing that the drunkenness still continued and that something more was to be got at, there was actually found a bottle of brandy tied in the same way round the

loins and placed between her thighs. Such is but an instance of the determination to obtain the wished-for supplies."

This, therefore, must be considered a diseased condition closely allied to, if not an actual form of insanity. Again and again memorials have been sent to Government, signed by the most eminent men in our profession here and elsewhere, expressing their opinion in these terms, "that habitual drunkenness is a disease closely approximated in a great number of cases to insanity, and susceptible of successful treatment." Whether arising from protracted vicious habit, or from constitutional organization, or some disease or injury, the craving for drink is an impulse as strong as that in the kleptomaniac, or suicidal or homicidal monomaniac; and while it differs from all other kinds of drinking, it is characteristic of a considerable portion of the ordinary insane, because, when under the fit, as there is a total annihilation of self control, the individual must surely be said to have lost the most distinguishing attribute of sanity.

Besides, in such cases there is evident proof that the morbid proclivity has an intimate connexion with brain structure and function, since it is found so often the outcome of HEREDITY. I have seen many, and I know of many more remarkable examples of this which I could quote if time permitted; and in life assurance investigations into family history I have found many instances of fathers, mothers, brothers, and sisters, grandparents, uncles, aunts, and cousins, having been intemperate in various forms and degrees. Also, it is well known, and I have seen many instances in the course of practice and in assurance examinations, of families thus alcoholically-toxically tainted having among their members those who were actually insane, or epileptic, or hydrocephalic, or affected with other forms of nervous disease,—inebriety thus producing in offspring its impress on the brain, which crops up in some form or degree, if not in the early stage of life, at least at some more advanced period. Nay, I have known mere children and those in early youth exhibiting the alcoholic propensity; and I have no doubt our psychologist Fellows now present must have seen frequent instances of this fact. I am sure also that they must have frequently seen the alcoholic propensity manifested in those actually insane, and could also give us some information regarding the worst types of dipsomania, who are the most troublesome inmates of any asylum.

As additional crucial proofs of the connexion of a drink-craving propensity with brain disorder, I would simply notice the fact of the former, sometimes occurring, in the worst degrees, from blows on the head, sunstroke, nervous shock from any cause, hæmorrhage, and some fevers. And, finally, on this branch of the argument I would notice the singular mental associations of habitual drunkenness with crime. These individuals, in police court language called "*habituals*," have generally a low mental development or twist.

They vibrate between our police court and the Calton gaol, seldom out of the latter many days or weeks, and that from year to year, to the great cost of the country. Regarding these psychological puzzles our excellent sanitary officer may, perhaps, give us interesting information, and also tell us if he has observed any curious uniformity in crimes committed under the influence of more or less drink. On this point, perhaps, I may be again permitted to quote from my evidence before the Select Committee of the House of Commons the following facts furnished to me by the late Sheriff Barclay of Perth.[1] He said: "Between the years 1844 and 1865 one woman was committed to prison 137 times for being drunk, and when drunk her invariable practice was to smash windows. Then there was a man who, when drunk, stole nothing but Bibles; he was an old soldier wounded in the head; when drunk, the objects of theft were always Bibles; and he was transported for the seventh act of Bible stealing. Then another man stole nothing but spades; a woman stole nothing but shoes; another, nothing but shawls; and there was a curious case (the indictment against whom I have) of a man, named Grubb, who was transported for the seventh act of stealing a tub; there was nothing in his line of life, and nothing in his prospects, no motive to make him specially desire tubs; but so it was, that when he stole, it was always, excepting on one occasion, a tub."

Now, an important question for consideration, after what has been said of the habitual drunkard's condition, is, Can it be cured? I unhesitatingly say that in a considerable number of cases it is curable; and that in a larger proportion, with suitable legislative arrangements, it might be. As in insanity, it is curable in the same sense that other diseases are. The more recent and acute the case is when taken under care and treatment, the sooner it is likely to be cured; while the more chronic and confirmed it has become, the more difficult will it be to accomplish that. So in habitual drunkards, from the imperiousness of the desire and habit, and the unwillingness to be under restraint, they are seldom brought early enough, if ever, under the necessary mixed medical, mental, and moral reformatory treatment; and as there is generally the greatest difficulty in getting them to submit to sufficiently prolonged control, and no power to enforce such, it is not fair to push aside as visionary the reasonable expectation which even the present results, under great disadvantages, fully justify. The experience of our private licensed homes or retreats in Scotland and England cannot be satisfactorily ascertained from the above causes, and the absence of Government or other inspection, and of statistics; but I know of several males and females treated in different establishments for considerable periods of time, who have done well afterwards. Even the lady whose case I read

[1] *Evidence before the Select Committee of the House of Commons*, 1872, page 50, answer 949.

to you, and seemingly so unpromising, was ultimately cured. A few days since I had a note from Dr Norman Kerr of London, the President of "The Society for the Study and Cure of Inebriety," regarding "The Dalrymple Home for Male Inebriates, Ricksmanworth, Herts," which home, he said, might be safely referred to from its being the only disinterested home under the Act (that is, not a private adventure), and which issues statistics annually, so as to be of permanent value. Dr Kerr says that "of the 115 cases discharged from the home up till 31st January 1888, 52 were then doing well. Of course the time is too short to justify the claim of all these as cures; but," Dr Kerr avers, that "from my experience, one-third is as large a proportion as can reasonably be hoped for in the case of males." As yet in England there is no licensed home under the Act for females, but there are seven for males. However, it is the general belief that with an improved law and suitable arrangements, the percentage of cure may be very considerable. I could quote in support of this opinion from a number of eminent and respected medical men in this country and in America, whose opinions are worth having; but I shall only now notice the opinions of two, who are well known to all of us. The one is that of Sir Arthur Mitchell, who said in his evidence before the Select Committee of 1872,—"We should hope to obtain a cure by prolonged compulsory abstinence under conditions favourable to health. Whether this hope would or would not be realized I cannot tell; our experience in the matter in Scotland is far from encouraging; permanent and satisfactory cures are certainly very rare; *but the experiment has never yet been quite fairly made,* and it cannot be so made without special legislation. If it were fully and fairly made, the expectation of good results, I think, is a reasonable one."[1] Then Sir Arthur goes on in the same answer to his questioner to say, that as to the comfort of such legislation to families, friends, and society, "there are no uncertainties." The other opinion is that of our friend Dr Batty Tuke, which I see in a Report of the Saughton Hall establishment for 1887, dated 1888, in which he states there were two dipsomaniacs treated under the voluntary permission law; and going on to speak generally of dipsomania, he says,—"Even amongst their number many have submitted to treatment for lengthened periods, and their subsequent history has shown that this intractable form of insanity *can* be permanently overcome. The records of this asylum show that many bad cases of dipsomania have been either cured or very materially relieved." This I consider as very valuable testimony, and I feel assured that Dr Clouston could corroborate the same. In regard to the American experience, about which much has been said in some quarters, I think we ought to deal with it generously, and without prejudice; for while there may have been

[1] Evidence before the Select Committee on Habitual Drunkards, 1872, p. 65. Ans. to query 1196.

some exaggeration regarding the percentage of cures, and confusion regarding the nature of the cases under treatment, as in our own country, where the homes are not licensed or inspected, the modes of treatment and the reports of success cannot be trusted; yet, on the other hand, I think there has been, as regards American institutions for inebriates, a very great amount of misrepresentation, for I know there are a number of excellent homes in the States and in Canada, conducted in the same excellent way as in our Dalrymple Home, under the superintendence of scientific, benevolent, and honest men, doing most excellent work. The American physicians have shown much more earnestness and more of a scientific spirit in the study and treatment of inebriety during the last twenty years than we have done, as the reports of their "Association for the Study and Cure of Inebriety" and their *Quarterly Journal of Inebriety* show ample proof. And from what I have been able to make out from these, they seem to have a fair claim to 33 per cent. of cures—cures as permanent as can be said of cures of any disease. But I hasten on to say—

2. That any arrangements for the establishment of homes, unless providing easy *voluntary* admission, and, if need be, *compulsory* enforcement and powers for detention, must cripple and almost nullify legislation for individual or relative benefit.

An important feature in the proposed Act, different from the present amended Act is, that *voluntary* admission is not to be public and deterrent, but private, simple, and easy. Instead of an appearance before a justice of the peace, magistrate, or sheriff, as at present, with two witnesses, and making then and there a declaration that he, the person, desires to be admitted to a home, as he *has been* and is a habitual drunkard, the transaction is proposed to be merely with the superintendent of a home (licensed, of course), which application, according to the schedule of the Act, is signed by the person, and attested by two respectable witnesses, engaging that he shall remain in the home, subject to the provisions, rules, and regulations of the Act, until discharged in accordance with the same. One of these provisions is that the person or "patient," according to the Act, shall remain at least twelve months under treatment, unless circumstances render it expedient that he should be discharged earlier.

We all know the difficulty of prevailing on a habitual drunkard to place himself under restraint. A propensity so deeply rooted in the constitution, so enslaving and irresistible, blunts the better feelings of human nature and reasoning powers as regards promised benefit, and incapacitates him from appreciating the advice, or to be moved by the entreaties, tears, or threats of friends. Thus in my own experience, after much interviewing and correspondence, even at the eleventh hour, when consent had been obtained and a home chosen, perhaps in only one out of a dozen of instances could submission be obtained. Therefore if persuasion proved ineffectual

in producing consent to enter a home, the compulsory enforcement clause which stands in the proposed Bill will settle the matter. This alternative hanging over the head of the person is therefore likely to make him prefer a voluntary surrender, which can so easily be made, to the ordeal of magisterial committal. The experience of such powers in Canada and in most of the American States to draft persons into homes, amply proves that while voluntary surrenders are very numerous, instances of enforced treatment are very few.

3. That the safeguards afforded by this Bill are amply sufficient for the protection of the liberty of the subject, and all the interests connected with individuals, families, and the public.

First of all, it is no small or unimportant feature in the proposed Bill that the *Board of Commissioners in Lunacy* is to be the *central authority* under the Act. The well-known constitution and character of that Board surely gives the very best guarantee for just administration. They are to have the *licensing* of all the homes; the sanctioning all the internal arrangements, rules, and regulations, not only for district homes established by public grants, but for all private homes, as they must all equally come under the Act. Then as to the safeguards connected with admission to any of the homes, district or private, the superintendent of the home to which the patient has gone must notify to the Board of Lunacy that fact within two days of entrance, accompanied with a full statement of the case; and should the Board not be satisfied with this admission, an immediate discharge will be ordered. Then in regard to a compulsory committal, the safeguards are, that before the sheriff will grant a warrant in any case, the applicant for that warrant must be a member of the family or a near relative of the person, or a friend taking an interest in him, or a magistrate in the interest of the public, setting forth in a solemn declaration the facts and circumstances of the case, accompanied with a certificate from a medical man, on soul and conscience, that he has seen the person within seven days; and if there are no private friends, then the certificates of two medical men. Then, of course, a patient, whether under voluntary or compulsory control in a home, has the right of appeal at any time to the Lunacy Board or the Secretary for Scotland for a discharge, if he considers that there has been undue or improper interference with personal liberty; or if he thinks he has good cause for complaint as to treatment received in the home; or some cause which makes it specially desirable that he should be discharged; or which discharge is urgently requested by his relatives or friends. Thus it will be seen that with all these precautions, and with regular *inspection* of all the homes, as in the case of carrying out the lunacy laws in regard to asylums, the best interests of individuals, of families, and friends are sufficiently safeguarded.

Of course, say what one may, there will be a hue-and-cry raised by certain people regarding an Act of this kind jeopardizing the liberty of the subject. However, as I have said elsewhere that it is certainly an overstrained delicacy in legislation which checks interference with a class of cases necessarily occasioning much private misery and public expenditure, as the records of the courts of law, the church, of our prisons, poorhouses, and lunatic asylums amply prove. Justice, humanity, political economy, and expediency all round, therefore, call for legal interposition and for facility to control and, if possible, to cure the habitual drunkard, since medical and other advice or moral suasion are of no avail in influencing his actions; and surely, when such is the case, it is the manifest duty of a wise government to exercise over all its subjects a paternal relationship. In a great many ways the liberty of the subject is most properly interfered with for personal benefit, for the protection or good of others, for the amenity of a neighbourhood, or the general welfare of the public; and why not in cases and circumstances so clamant as those pointed out?

4. Lastly, the defect that I see in this proposed Act is the absence of provisions reaching down to the labouring, the pauper, and the criminal classes.

It was thought, however, better in the first instance to seek legislation only for such as were able to pay board for treatment in the restorative homes, which would thus to a considerable extent prove self-supporting, otherwise the cry against increased taxation might shipwreck the proposed Act. I hope, however, that ere long the Act may be extended to such classes, for whilst the vice of drunkenness in all its most degrading and disgusting forms is more prevalent in the lower strata of our population, disturbing peace and prosperity in private life, and endangering the safety of the public, there are in it also a greater number of the worst type of inebriates, namely, genuine dipsomaniacs, dragging down to beggary and wretchedness numbers of those who are well-to-do, and thus largely increasing disease, destitution, and crime, and consequently continuous gravitation to our hospitals, poorhouses, asylums, and prisons, imposing a correspondingly heavy burden on local taxation and the funds of the nation.

Of course private enterprise or philanthropic associations cannot be expected to establish inebriate homes or sanatoria so as to meet altogether the exigencies of this great social evil; but municipal and parochial authorities, perhaps supplemented to some extent by government, could accomplish most excellent results. From work done and wages earned by the inmates of such institutions, the expense of maintenance might to a large extent be met and something over and above gained for the benefit of their families, or, in the absence of such, for his or her own use when the period of control terminates; thus also habits of industry and providence cultivated, would prove excellent counteractives against a return to drinking

habits; and other agencies—physical, mental, moral, and especially religious—would be the surest means of generating self-esteem, and strengthening the power of self-control.

Then as to the large, troublesome, dangerous, and expensive class of inebriates, so well known to our magistrates, police, and prison officials, as criminal drunkards, government ought unquestionably to make some provision in a Habitual Drunkards Act, by which suitable treatment could be carried out in reformatories either in connexion with or altogether distinct from prisons. Inebriates of this class are at present almost irretrievably sunk in the lowest depths of the social scale. They are almost constantly resident in police cells or prisons from oft repeated sentences on account of assaults, or crimes, committed to obtain drink or under the influence of it; many of them are most dangerous, and all are pests in society, and, as must be admitted, most costly to the country, while it is notorious that not the smallest benefit is produced by imprisonment.

Of course in such establishments the punitive element could not altogether be separated from the reformatory; and the expense of upholding them would to a considerable extent fall on prison boards; but I firmly believe that the good accomplished in them by strict, yet kindly and judicious management, there would in time be ample compensation to the State, and probably a large saving of the at present utterly useless expenditure. Here inmates would be obliged to work, in the first instance, for their own maintenance, and possibly by good conduct win something over to help themselves when the term of restraint expired. Direct commitment by the magistrates to such reformatories might in many instances be judiciously made after three or four convictions without passing the criminal through a prison, which all experience has shown to be utterly useless as a preventive of future offences, and a monstrous waste of money as regards this class of offenders. But if it must be continued so to some extent as a mark of justice on account of crime committed, the prisoner might be transferred to the reformatory in some cases before the period of sentence expires, or at any rate then, by a warrant from a magistrate or sheriff for such prolonged detention in it as circumstances justify, when he would be subjected to those various influences already spoken of. By such means I firmly believe a considerable percentage might be saved from an otherwise almost certain lapse into the old evil ways, and a speedy return to prison life, or a curse on society. Without pursuing further this important branch of my subject, I would refer to the evidence I gave before the Select Committee of the House of Commons in 1872,[1] and especially to the sugges-

[1] "Report from Select Committee on Habitual Drunkards, together with the Proceedings of the Committee with Minutes of Evidence ordered by House of Commons to be printed June 13th, 1872." See evidence given on March 19th, pp. 48–57.

tions for legislation, which that Committee did me the honour to accept, and insert in full in the Appendix to their Report (No. 3, pp. 186-190).

Mr Taylor Innes, advocate, said, in the few words which he would say he would limit himself to the subject which Dr Ritchie had been good enough to draw his attention to. The liberty of the individual was the obstacle which might be supposed to stand in the way, and which had to be attended to. He had a strong belief on the general question that this ought to be no difficulty in the way of passing some such measure as they were now contemplating—a mere measure for the compulsory detention of dipsomaniacs—nor did he believe that public opinion would find that now a serious difficulty. He observed that Dr Yellowlees had very little hope with regard to this matter, and undoubtedly there had been difficulties in the past. He thought that now, provided that the matter were carefully looked at in detail, the public and the House of Commons could be got to face a new measure and to pass it without serious difficulty. In America the liberty of the subject was put first: yet such legislation existed. And in Britain he thought that the state of feeling with regard to the mischief of drunkenness, and this class of cases in particular, was such that it should not be looked on as hopeless to get such a matter approved of by the community. Of course that was only upon the understanding that all precautions were taken, in the particular Bill, so as to satisfy the public conscience that there was no restraint of the liberty of the subject except what was necessary. Mr Morton had done wisely in publishing the Bill, and in submitting it to bodies like the Medico-Chirurgical Society, in order that discussions might take place, and that legislation might be prepared for. With regard to the measure he had a word to say. The main point in which it (the draft Bill) differed from Dr Cameron's Act lay in its giving power to a man's family, as well as to himself, to propose detention. He thought the intention of this was good, but he thought great care would require to be taken, and he was not sure that there were no mistakes. He found, for example, that no intimation was proposed to be made to the man when one of his family, with the consent of a doctor and one or two friends, wished to have him detained. He did not think they would get the public to consent to that, and he did not think they should. He said they ought to give full and fair notice to the man that they intended to detain him. The Bill spoke of "families" and "relatives." These were words which would require definitions, and other clauses should be amended. Twelve months was the time stated for the detention. That was a hard and fast line. Was there, he asked, any medical reason why twelve months should be the time? Some cases of dipsomania would require more: twelve

months in many cases would be of no use. He should think there would be cases, again, of men in business who would find it difficult to get away for more than, possibly, six months. He thought, however, that was entirely a medical question, and it should be in the medical certificate which was to be granted. The medical certificate should state what amount of time was necessary in the medical view for recovery. Lawyers in this would defer to the medical men's opinion, and the relatives also would be willing to follow their advice. His suggestion was that there was no necessity for having a hard and fast line. There was no hard and fast line in the Habitual Drunkards Act, whose three, six, nine, or twelve months might, he thought, be extended. That led him to the following. He observed that it was proposed to repeal the Habitual Drunkards Act and to put this new Bill in its place. He thought they should consider well before proposing or pressing that. There was an elaborate definition given in this Bill of the class of persons whom it was intended to deal with, but it struck one who was not a medical man as an artificial and difficult definition. They would find it difficult to push it through the Commons, who had already passed a much wider definition embodied in the title of the "Habitual Drunkards Act." He did not think they should attempt to pass a wholly new Bill with a wholly new definition. He would suggest that it would be well worth considering whether, retaining the Habitual Drunkards Act, they might not add to it a clause giving power to the man's relatives, as well as to the man himself, to propose and bring him forward for detention. He thought they would find it much less difficult to persuade the House of Commons to add to the Bill they had already passed, than to pass a Bill with such an elaborate preamble and such a definition. He was glad that it was not proposed to press the assessment clauses, but in the meantime only to shut up self-supporting people. It was, therefore, only a sort of tentative measure, and he thought if this succeeded, they would have a much larger problem to face with regard to the working class, the criminal, and the pauper. However, it was well worth their while trying an experiment. But that was another reason why he thought their experiment might be tried in the way of adding to the Habitual Drunkards Act, and why they should not try to pass an elaborate definition and a new Bill. In any event, he said, they were much indebted to Mr Morton for urging the question.

Dr Batty Tuke said he felt difficulty in rising to speak on the question, because, not being so hopeful as the speakers who had preceded him as to the effect of legislation, it might appear that he wished to throw cold water on the whole subject. That was not so: no one knew better than he the misery caused by habitual drunkenness, and no one was more desirous than he to find some

means of mitigating it. But he was most anxious that the Society should look the many inherent difficulties of the whole question straight in the face, and that it should look ahead and endeavour to foresee what would be the outcome of the working of a legislative enactment such as that then under consideration. Dr Peddie might be congratulated on stating the position regarding drunkenness in a scientific manner, inasmuch as he had pointed out that drinking was one of the manifestations of many forms of insanity. But he (Dr Tuke) was not prepared to admit unreservedly that insane drinking or dipsomania was synonymous with habitual drinking, or that habitual drinking was necessarily disease. He drew a distinct line between vicious drinking and insane drinking, and it was only in the latter condition, insane drinking, that he thought they had any right to ask for legislative measure of control—control meaning seclusion from society. The first difficulty arose out of the question, Who should be the active agent in procuring such control? Was it to be the doctor who was to decide between vice and insanity? He thought that was too much responsibility to place on the shoulders of any one man. Where there was such an open question between the physician and the casuist, he thought it was one that must be determined by more than a single individual. Was it to be a member of the family of the inebriate? This would be to adopt a *lettre de cachet* system open to gross abuse. His opinion was that any such matter must be determined by a court. His old friend and master Dr Skae had long ago suggested as a proper court to determine between vice and insanity, one consisting of a sheriff of a county, assisted by a medical assessor, a representative of the inebriate, and a representative of the family. He could not think that the public would ever consent to the liberty of any individual being curtailed by a less responsible tribunal. He thought that this court should be invested with considerable powers. If it found that the inebriate was an insane drinker, it should have not only the power of deciding what length of time the patient should be subjected to seclusion, but should also be the only agency by which the length of what was practically a sentence should be curtailed. One of the great faults of all the Habitual Drinkers Acts had been the great ease by which an inebriate could obtain release. In Mr Morton's Bill he saw it was proposed that a Commissioner in Lunacy should have this power. He thought that this should not rest with a single individual, and that the court, however constituted, should be the only agent by which its decision could be modified. He thought that whatever representation the medical profession made to the Legislature or the public, it should make a very full representation of the medical facts; and one fact especially should be put prominently forward in order to avoid anything like reflections in the future. That fact was that insane drinking cannot be cured in a short period of time. Every physician who had any experience of

the condition knew that it was useless to speak of six months, or even of a year, as the period of seclusion. It was a well-established fact that, in a very large proportion of cases, it took at least two years to give the patient a chance of recovery, which, he presumed, was the main object of Mr Morton's Bill. What he wanted to impress upon the Society was, that in any representation it may make as a medical body, this important fact should be put prominently forward. He would like to ask the meeting to consider whether it thought the public would ever consent to allow a man to be incarcerated for at least two years for drinking, on such slight authority as was suggested in the Bill under consideration? In his opinion it was extremely doubtful whether such powers would ever be granted to any court, for, in point of fact, a man might be incarcerated for life. They had been told that only 30 per cent. of cases submitted to treatment in homes recovered. What, then, was to be done with the large balance? Were they to be committed and recommitted? If not, what was the good of the Bill so far as 70 per cent. of those to whom it would apply was concerned? This would be a great stumbling-block in the face of any legislation, and it was one which he would urge on the Society to consider carefully before they passed any resolution based on Mr Morton's Bill. He would remind them of their experience of public measures which had been passed by the Legislature in consequence of medical representations. He referred to the Contagious Diseases Acts, which were adopted on much more definite data than those of any Habitual Drinkers Act. John Bull was touched in his pocket, a very tender point. Repressive Acts were passed, were set in action, worked beneficially, and yet were swept away on purely sentimental grounds a few years after their enactment. What, he asked, were they to expect were analogous measures adopted with regard to drinking, which a large section of the community regarded as no very serious evil, and which would be hard to convince that it was the outcome of insanity? Then, again, if an Act was passed, what was to be done with the inebriates? To what institutions were they to be consigned? It was evident from the public prints that private enterprise had for the most part failed. In England it had met with but slight success. He was glad to see that the clauses in Mr Morton's Bill, which suggested that the poor ratepayer was to be assessed in order to provide a home for the well-to-do inebriate, had been withdrawn; but he could not help pointing out that such a suggestion, emanating from such a man as Mr Morton, was strong evidence of the inherent weakness of the scheme. He thought if any Act was ever passed, insane inebriates should be confined in special departments of public asylums. They were the proper institutions to undertake their care, and there would be less chance of abuses arising out of such an Act if worked through the instrumentality of public institutions. But supposing they had obtained all this

machinery, to whom were they going to apply it? He very much feared that its action could only be brought to bear on the ultimate stages of alcoholism. Could any one suggest a plan by which it might be made to apply to the earlier stage, the only period in which it could be materially useful in a curative point of view? And then from the preamble of the Bill, and from what had dropped from previous speakers, the Bill was only to apply to a certain section of inebriates—those who by their evil habits were liable to ruin, or compromise the comfort of their families. Now, how would this apply? It would apply to the man of independent means, who might squander his capital; but how would it act in the case of a professional man who, by being shut up, would have his business ruined for him? Then, again, if the definition of an inebriate such as was set forth in the Bill was adopted, it would apply to hundreds and thousands of cases, and homes without end would have to be constructed. Further, why should ruin to families through drink be alone provided for? If it was a main object of the Bill to provide against family ruin and scandal, why not extend its action to other vices, such as lechery and gambling? He could not believe for a moment that any Bill could ever pass Parliament warranting the incarceration of habitual drunkards in the lump; he doubted very much if any satisfactory measure warranting the treatment of insane drinkers would ever be passed; but he would be willing to support such a measure, because he knew it could be advocated on well-established medical data. But he deprecated as an utterly Utopian scheme any direct interference with the vicious inebriate. The only tangible means by which the medical profession could hope to affect that vice would be by throwing its great weight into a measure providing for rational reform in our licensing laws.

Mr Cooper, advocate, said he was present at the request of Mr Morton. Mr Morton sent the Bill to him, and requested that he should be there and say anything that suggested itself to him, upon rather the legal aspect of the Bill than the medical. Upon the medical aspect he was incompetent to speak. Upon the legal aspect of the Bill he would venture a few criticisms. He felt his position rather a difficult one after the destructive criticism that had just been pronounced by Dr Batty Tuke; and if he might criticise that criticism at all, it would seem to him to be that Dr Batty Tuke objected to the Bill because it could not do everything. It has been a motto in British legislation for hundreds of years past that it was better to have half a loaf than no bread. Although they were not able to root out drunkenness altogether by this Bill, still they hoped to be able to do away with it to some extent, and to alleviate the sufferings which resulted from it. Dr Stewart and Dr Yellowlees spoke with great admiration of the preamble of the Bill. He felt sorry to suggest that the preamble perhaps was, as

it existed at present, a little out of place. He thought that this very careful definition of a habitual drunkard which was given in the preamble would be better stated in the definition of the word patient. The preamble was a part of the Act of Parliament. He thought it was an awkward part in which to place an important definition; and if he had had the framing of the Act, he would have suggested that the first part of the preamble should simply read that there was no adequate provisions made in the Lunacy Acts for the proper treatment of patients. There was, he thought, no reason why the definition which was given in the preamble should not be stated in the third clause, where the word patient was defined. He thought that that would put the Bill into a proper form. With regard to the remark that narcotics did not seem to be included by Mr Morton, he noticed that in Schedule E, giving a form of declaration by an applicant for the detention of a person, the word narcotic was applied; and he thought there were other sentences in the Bill which plainly indicated that Mr Morton had in view persons suffering from the habitual use of narcotics as well as of alcoholics. He would not trouble them with the minor alterations, which were merely verbal and formal. He thought that the fear thrown out by Dr Yellowlees with regard to the Lunacy Commissioners not being ready to take up this duty was somewhat odd. The Lunacy Commissioners were the servants of the public, and he did not think they should complain for having put upon them this work, which could be a very slight addition to their present work. Dr Tuke referred to the enormous difficulties that would result in the carrying out of this Act because of the great number of persons who would become candidates, either of themselves or by their relatives, for admission into the restorative homes; but, especially since the taxing clauses had been cut out of the Bill, it should be remembered that the only persons who would really get admission to these homes would be persons who could not only pay, but could absent themselves from business for a period of a year or more. It would not be those who had to earn their daily bread. In fact, the Bill, especially after the clauses about assessment are cut out, would only be applicable to persons suffering from habitual drunkenness, and who had sufficient money to keep them in the homes for the necessary time. The only other remark which he wished to make was about the clause of application for detention by relatives. He quite agreed with what Mr Taylor Innes had said, that he did not think Parliament would look at the clause if it did not provide for intimation being made to the person whom it was desired to detain; and he thought there was in the 19th clause, in the very last sentence, a very dangerous provision made. It read,—"It shall be lawful," etc. If they passed that clause, every person who wanted to get out of the homes would make a disturbance, and would at once be put

out. Another provision he would suggest,—that if a person detained in a home refused to allow his funds to be paid over for his maintenance, application could be made to the Court for the appointment of a judicial factor, who would during the time of the person's detention have control of his funds, so that they could be paid to the people who were maintaining him in the home.

Dr Littlejohn would have preferred to have waited till some of the specialists had spoken, but having been called on by the chair, he would answer the question put by Dr G. Stewart. He (Dr Littlejohn) had never known of a single case of cure having been effected by residence in jail. The reason why was, that the sentences were too short. The appetite for drink was only whetted by this temporary seclusion, and the prisoners, on regaining their liberty, fell easy victims to their former excesses. With reference to Mr Morton's Bill he had a strong feeling that it did not go far enough. The time had gone by for special class legislation. The franchise had been extended, and the working classes were now fully alive to their privileges, duties, and dangers. Were they at this time of day to legislate for the upper ten thousand who were far more able to assist themselves than the middle and lower classes who in the matter of this terrible disease were practically helpless? He saw a great deal of this form of insanity among the poor. He had the curiosity to ask the Clerk of Police in this city how many of such cases might pass before the inferior Court every year. On consulting the records, it was seen that at least fifty cases turned up annually who were fit subjects to be dealt with as proposed in Mr Morton's Bill. Now, why should not these cases be dealt with by such legislation as was proposed for the better classes? They spoke of the liberty of the subject, but the whole of the tendency of the law at present was to interfere with that liberty if the public safety, or even the well-being of the individual, was in danger. They had reformatories where children were confined and brought up as useful members of society. A child who committed theft, or was not under proper control, was taken from his parents for a series of years, and society approved. Then there was the Infant Protection Act, which insisted on compulsory registration; and evidently, after the painful occurrences of the last few weeks, it was evident that the stringent provisions of this Act would be considerably extended. Lastly, with regard to the difficult subject of infectious diseases, parliamentary committees had decided that for the public safety it was expedient that such cases of illness should be promptly notified to the authorities, and, whenever necessary, isolated in an hospital. He was, therefore, clearly of opinion that it would be safe to apply to Parliament for an extension of the principle of this Bill to all classes of the community, and the evil being recognised, everything connected with the machinery of the

Bill should be open and above board. It would be hazardous to allow a man quietly to place himself in these homes. Any one might give a false name, and thus obtain temporary retirement to suit his own purposes. He thought it important that all such parties should feel their position, and while undue publicity might be avoided, no person should be admitted to these homes unless he had gone before the sheriff, and had his statement formally made and recorded that he was voluntarily depriving himself of his liberty for a certain period of time. He felt strongly on another point. He did not know, but he would like to ask Drs Tuke and Clouston if they had power to compel their patients to do any work if the patients claimed to remain idle. [Dr Tuke—We have not the power.] Now, he (Dr Littlejohn) would make work of some kind or other a *sine quâ non* in these homes. Without healthy exercise and work it was impossible to effect a cure of this disease, and the superintendents of these homes should be empowered to fix the kind and amount of work for all patients of both sexes under their charge, and to see that it is done. The occupants of the homes might get a portion of the produce of their work, the rest going to diminish the expenses of the establishment.

Dr Clouston said he was well aware that one's remarks at that late hour must be very concise. When he came there he thought they were not to discuss Mr Morton's Bill so closely. He thought they were rather to discuss the general question, and to give their opinions regarding it. To the general question he would therefore confine his remarks. In any action they might take, they would be wise, in the first place, to limit such action to that form of drunkenness which they could scientifically reckon to be disease. He thought the more quietly and the more gently they insinuated the thin end of the wedge in this matter through the House of Commons the better. In the long run they would reap the greater benefit. He scarcely believed that the definition which had been given in the Bill was a right definition of even diseased drunkenness. He would say it would be wiser to accept a general term, such as habitual drunkard. It would be a simple matter to answer, How was drunkenness related to disease? but more difficult to reply to the question, How could they make it out to be a disease? He thought the very latest investigations into the functions of the brain proved the relation, and the latest facts in regard to the disorder of the higher brain functions all tended towards the idea that the practice of alcoholic drinking in certain cases is a disease. In the first place, it was strictly analogous to other insanities. They had no difficulty when a man intellectually was so far different from his fellows that he believed things that no other man believed, in saying that that man was diseased. If he entertained delusions of suspicion in regard to actual matters

of fact, believed matters of fact to be not fact, they had no difficulty in saying that that man laboured under a disease. That man might be in his affections and in his will in a wonderfully sound condition, and even in his conduct he might not be far wrong, still they called it disease; and in the same way, if a man changed in his affective nature, or if a mother totally changed her affections, they had no difficulty in saying that it was disease. If a man changed in regard to his still higher functions of inhibition or volition, they had no difficulty in saying that he was an insane man. When a man was so diseased in his impulses and in his will or inhibition that he had irresistible impulses towards suicide, no medical man hesitated to say such a man was diseased. If a man in certain other circumstances had changed in his higher inhibition—say when he has an irresistible craving for alcohol or morphia, or cocaine or chloral—why should that man be regarded as not insane? Why should such a man—especially if corroborated by other circumstances, *e.g.*, if you find him belonging to an insane family, if you find a man taking this craving at certain times, and being under its influence at certain times and free from it at other times—not be regarded as diseased? From the time he had the craving he exhibited one of the symptoms of the higher brain diseases in the shape of sleeplessness. He thought the more they knew of the brain the more certainly they were led to the disease theory on purely scientific grounds. It would be said by some that the disease was brought on by the patient's conduct. This was unquestionably true, but he asked the members of the Society, as medical men, if it was not the case that a great many diseases are not brought about by evil conduct, and which the sufferers could have helped if they had liked. Did they hesitate in classing epilepsy as disease because the man originally indulged in such conduct as brought on this complaint? Dipsomania might be regarded as disease, even though the man himself had caused it. He took it that they should legislate on the assumption that they had a disease to do with. It might or it might not be the case that disease was present in any given drunkard. In regard to the causes of dipsomania, they knew that many cases resulted from the action of causes that caused ordinary nervous diseases—from blows on the head, from loss of blood in many cases, from exciting labour, from living under bad hygienic conditions. All these were causes of ordinary disease. Hereditary drunkenness was closely allied to nervous disease. It was mixed up in many ways with real insanity. They ought to legislate for such people. Dr Batty Tuke had said that ordinary public asylums were the proper places for them. He regretted that in this he differed from him. It had been his experience—and he was surprised it had not been Dr Tuke's—that persons labouring under this disease did not do to mix with ordinary insane people comfortably and for the good of both; and

there was another reason, which had been alluded to, that they could not detain them long enough to do any real good. In a case of ordinary mania they could only detain him just sufficiently long to see that his convalescence was secured. They could not detain such a case a year or two after the symptoms had disappeared. This was a test by which the dipsomaniac was to be distinguished in regard to his treatment, and this was one reason in addition to that mentioned by Dr Yellowlees why they should not be sent to the public asylums. So strongly did he feel on this point, that he never took in a dipsomaniac into the asylum if he could help it. He refused all such cases, because he knew he could not do the man the good that was necessary, and therefore he thought there was no good in taking him into the institution. He thought it was now time something should be done in this matter. It had been long evident to all the profession that something ought to be done.

In regard to the question of cure, he did not think they were in a position to express dogmatic opinions. They had never had the opportunity of putting a man under the proper conditions. He thought they must face not only the case of the confirmed drunkard, but they must ask for power to take care of the drunkard at the early stage of his disease, and at that stage place him under proper treatment, a more difficult matter from the legal point of view. If this Bill had been only to affect the confirmed case he would have taken little interest in it. Such confirmed cases were nuisances to society and their friends. Perhaps they did not deserve a great deal of money spent on them. But what of the relatives of dipsomaniacs and society generally? Had they no claims? Surely they had. Should a woman, because she had had the misfortune to marry a dipsomaniac, neither have the power to try and cure him nor to leave him? Many of the medical profession believed that the disease could be cured in the earlier stage. Was there any other disease whose proper treatment was put off till it was confirmed? There were no doubt many objections to be faced, such as that of the liberty of the subject. He thought, however, they were entering on a time when the liberty of the subject would be less thought of than it had been in past times, because there was no real risk to it now. That was a lesson they were learning from the American legislatures. They took care that the liberty of the subject did not interfere with the working of the body politic. In this country they would come to that. If the dipsomaniac interfered with society, he and his liberty would have to go to the wall. In regard to whether they could distinguish between vice and disease, he was quite ready to admit that it was an exceedingly difficult distinction in many individual cases.

They must also face the difficulty mentioned by Dr Littlejohn. What was to become of the poor diseased drunkard? The only

way he could think of at the present time was to call on the public to subscribe money to provide these homes for the poor. The rich could help themselves. They could provide homes for themselves. He did not see why, as the Legislature taxed us for reformatories, they should not give the diseased drunkard in the early stage of his career a chance. Of course, they did not like to be taxed any more; but the inmates of the homes could help by working for their livelihood, and in that matter of work he most strongly and earnestly confirmed what Dr Littlejohn had said. They would never cure a drunkard unless they provided him with employment and he wished to be cured himself. He thought the best suggestion he could give at present was that they should try and get the drunkard at the early stage away to some Highland home, where he could get some real hard work to do, and where he would live in a high moral atmosphere. He said the atmosphere in which a dipsomaniac lived was most important. And for this reason these restorative homes should not be too large; they should not put too many of these people together, for they corrupted each other. They must restrict the size of them to do good. To put anything like 200 together would be outrageous. They would have to restrict them so that the domestic character would have to be preserved, and the feeling of control and direction on the part of the head would have to be preserved in regard to each individual.

He had only now to make two suggestions in conclusion. First, that this Society petition generally in favour of new legislation for the habitual drunkard. He thought they might take it for granted that the House of Commons and the House of Lords and their various committees would see that the Bill was not dangerous to the liberty of sane people. He had one more suggestion to add, to which he attached the greatest importance, viz., that a clause should be added to the Bill making provision for the compulsory detention of diseased drunkards in ordinary houses, with suitable guardians, in suitable parts of the country, under proper guarantees and under proper inspection. When they sent these diseased drunkards away to ordinary homes, they should be under proper superintendence, and the guardians should have a certain authority over the patients. There might be a difficulty in getting proper homes built. There was no difficulty in getting a Highland farmer or clergyman for £50 or £100 a year to look after a man.

Mr Shaw, advocate, thanked the Society for the honour they had done him in asking him to the meeting. He did not intend to make any observation, further than to say that he agreed almost entirely with the remarks of Mr Taylor Innes. With regard, however, to the preamble of the Bill, he rather differed from him, and he also differed from Mr Cooper. He thought they

should leave out altogether the proposed preamble, which was academic rather than practical, and he would not adopt Mr Cooper's suggestion to put it into any other part of the Bill. A wiser proposal, he thought, than supporting the present Bill, which had been so carefully drafted by Mr Morton, would be to secure an addition to present legislation in the form of compulsory power, as provided for in what is practically section 17 of Mr Morton's Bill. The Habitual Drunkards Act of 1879 contained in section 10 a permissive clause, and they found that the almost entire and sole objection was that that clause had been insufficient for the purpose aimed at, as indeed any one would suppose who would read it. What they now wanted was mainly to get the compulsory power added to those at present existing by Statute. A bill presented to Parliament containing such a preamble and provisions so elaborate, which might be the means of causing needless debate in the House of Commons, would be unpracticable in the present state of public business. Any scheme emanating from this Society would be much simpler, if it merely proposed to add a compulsory power to the present permissive clause,—in fact, to dovetail section 17 of Mr Morton's Bill into section 10 of the Act of 1879, and not to trouble about preambles. He did not quite agree with the views expressed by Dr Tuke. He had come for instruction; and he confessed that he went away with a profounder conviction than ever that in the case of men who are subject to this disease, whether inherited or whether caused by actual vice in their individual history or not, they must deal with these men as not only being harmful to themselves, but as requiring to be treated by public authority in the interests of society, because they were the pests of society.

Dr Connell, Peebles, said some persons might wonder why he was present. He had come simply to give his experience. He had been Medical Officer for twelve years to one of these restorative homes, viz., the Brownsland Home; and during that time he had managed to form some opinions and to collect some facts. He had heard some remarks and opinions which were wide of the mark. He thought it right, therefore, to give them a few grains of fact.

In the first place, he might mention that the home he spoke of was certainly not for the upper class. He had a copy of the rules and the last report in his hand, and the first sentence was that this institution is "intended for women who have fallen," etc. The expense of staying there was not large. They paid only 8s. per week. He did not say, however, that that paid the expenses. The difference was contributed by a number of philanthropic ladies and gentlemen in Edinburgh and elsewhere. 122 people had been treated in the home within the last twelve years. They had resided in the home for various periods. Some had resided a short period,

and others a much longer—from two months to three and a half years. They had a lady still who has been there three and a half years, and who was most anxious to continue at the home, because she did not wish to go back to the temptation. Of these 122, 44 were reported as having done well. Some had broken down; 1 had been very hopeful. One of the women treated was the wife of a Liverpool captain, and she resided for fifteen months in the home. She had then gone back to her husband, and had been a good wife for the last five years. These were facts which spoke for themselves. He thought there had been a great deal too much pessimistic criticism, especially that indulged in by Dr Tuke. Some one had remarked that Dr Tuke's objection to the Bill seemed to be that they could not get everything by it, but still surely they would get something. He thought it quite warrantable to go in for such a proposal as was made in the Bill. He thought it right also to say concerning the cases in the home he referred to, that they had never had one single case of acute illness, and some of the women had been exceedingly bad drinkers. They had spent the exceedingly cold winters of 1879-80, in which year the temperature at Peebles was on two occasions 14° below zero; and the years 1881-82-83 were exceedingly cold, and for eighteen weeks that year they never saw mother earth, and yet they lived without stimulants. He had only refused one case. She came to the home in the winter, and he would not take the responsibility of treating her through a cold winter without some stimulant; and as the Committee did not allow that, she was sent home. He had never heard anything further of her since. In the home they had women varying from nineteen to sixty-nine years of age, and they were chiefly of the poorer classes. One woman was the widow of a doctor in the Indian army. One was the sister of a doctor who supported her, and, with the exception of one or two others who had seen better days, they were all the wives of working men, such as tin-plate workers, engine-drivers, and such. It was not, he thought, the case that they would have difficulties in legislating for the poorer classes. All that the matron and the master of the home felt they required was simply the right to keep these people there. The house where they were kept was as private as possible. It was 3½ miles from a railway station, 7 miles from a town, and lay between two valleys, and high hills on all sides; and the road leading to it had a long slope, so that no one could escape without having two or three pairs of eyes fixed on them, and thus being detected. There had, however, been escapes. Two or three women had escaped, but only to wander for a while on the hills and to return to the home. A single case of suicide had occurred, which he did not attribute to the want of drink. She had shown signs of it before. It was a case, he said, which would not have been received if it had been known. One of the conditions was that mental alienation should not have been shown. It was a fact

that these people in the home had done well without alcohol. It was not the case that they would contract disease. Of course if there were cases where alcohol was necessary there would be no difficulty as to arranging. He had one thing to say about habitual drunkards, and he would say it in plain English. It was that when a man or a woman reduced him or herself to the level of a beast, then he or she deserved to get the treatment of a beast. Though he said this in such rough, harsh language, he did not mean to be unkind. None of them, he said, would be unkind to their beasts. He meant simply that when these people did as he had said, they should be reduced for a time below the level of manhood. That, he said, was a proposition which none of them would refuse. There was another proposition which he could not help formulating. It was, that it was not only lengthened residence in these homes that would do good. He would have been surprised if Dr Littlejohn had known of any cases of cure. A psychological reason for a year being the minimum length of time was that in the diseased condition of some of these people the coming through a certain number of months, through a cycle of the seasons, would act as a support; and if they have kept it through the various seasons they then propose they might venture back into the temptation. That was, of course, only his own opinion. Another thing that he desired: he was a pessimist in the matter of curing the patient by merely length of time. How many, he asked, even after this period, should be trusted again to give entire satisfaction? These institutions, he said, must be conducted, as this home was, upon Christian principles. He thought Dr Littlejohn's proposal as to work being compulsory was a good one. The only thing that would raise them was to instil into them Christian principles by a sound moral training, and even then he would say that the Church could never quite trust them again to themselves. They would require to be kept out of temptation. They required to put a fear and dread of it into them, such as they would have at poison. He thought doctors could do a deal to effect a cure. They should never carelessly prescribe stimulants to any one. He knew the case of a young man. He had kept away from it for eight months, and then he went and took the advice of a doctor. The doctor prescribed stimulants, and he has lost hope of him since then. The only hope he had was if the people kept rid of it entirely. He looked forward to the passing of such a Bill as this, and he hoped it would pass. It would do an immense amount of good. He had a firm conviction that if they were to keep them in these homes they must make them work, and another thing, he said, was that you must not make the homes too large. Theirs was not too large. It had eleven rooms, and there were twelve women there at a time. They did needlework and all kinds of sewing, and garden work when possible; and, he said, that during those twelve years he had been connected

with the home he could look back on all the people who had been in the home, and say that there was scarcely one that had not been improved by their stay there.

Dr Clouston asked, How far off was the nearest public-house?

Dr Connell.—The nearest one was 7 miles. Carts and vans used to come, and occasionally brought liquors, but they were now kept in quarantine, and not allowed near the home.

Dr Strachan, Dollar, said they were all agreed as to the great necessity of doing something in the case of habitual drunkards. They were the pests of society, and should be shut up. He thought legislation would be well applied if they could carry out the suggestion of Dr Littlejohn. He, however, was doubtful as to the cure which could be got by such lengthened incarcerations. After any one had been in these restorative homes and gone out, it was absolutely necessary that they for the rest of their lives should be total abstainers. They could not venture at any future time to taste at all of alcoholic drink. That was his experience. He had known men for years total abstainers, and who began to feel themselves strong, and returned to the temptations and began to take a little, and as sure as they did so they fell back to their old ways. He, however, considered that this principle of total abstention could be put in practice without that long incarceration. That was his own experience. He was not a teetotaler himself, and he did not advocate teetotalism for every person, but he certainly did advocate it for habitual drunkards. It was their only safety and cure. He thought some good might be done by their going into this incarceration at the earlier stages. He would mention a lady patient of his, whom he said was as bad as most cases. One fact he would mention, the W.C. in the house had got stopped up. They could not understand the reason, and sent for the plumbers to examine it, and it was found stopped full of *Eau de Cologne* bottles, the contents of which she had been drinking. He thought she must be considered to have been a pretty bad case. That lady consulted him, and he put it before her, that her only chance of getting over it was total abstinence, and he strongly advised her to become a teetotaler. She took his advice, and has kept to it for over twelve years. And he would press strongly upon all medical men that that was a very important matter. These people required to be protected from their friends. It was through the inducement of society to take a little for their stomach's sake that brought back the craving again. He would again strongly urge upon medical men to get their patients to become total abstainers, and to take an active part in the movement. He thought that could be done quite well without legislative incarceration. There were a proportion of people who were liable to this disease, and the ordinary customs of society led them into it. It was not the

lighter drinks which produced the disease and did the harm. It was the strong drinks, such as whisky and brandy and gin. These were the drinks which produced the craving. They excited the nervous system for the time with consequent reaction for which the people craved relief. He would look upon it not as an indulgence, but as a craving for relief from a great suffering. He would suggest whether it would not be possible for this Society to try and get these strong drinks classed as poison, and sold like laudanum. If laudanum were sold as the strong drinks were, they should then have the opium habit as productive of evil as the alcoholic habit was now. If they were to restrict the sale he believed they should do away with this disease of habitual drinking. That was the direction in which he should like to see legislation taken, and he was glad of the opportunity which had been afforded him to put this before the Society. It appeared to him that the advocates of abstinence went too far. Comparatively little opposition would be had to this restricted legislation in the matter of drinking.

Dr Andrew Smart said that, on account of the lateness of the evening, and inasmuch as the question had already been very fully and ably considered by previous speakers, he would only add a few words in explanation of his experience of the treatment of alcoholic cases as carried on in the ward under his charge in the Royal Infirmary. During the three years he had been in charge of Ward VI.—a ward, he should explain, for the reception of acute and chronic nervous diseases, but better known as the "D.T. Ward"—there had been in all 1770 cases under treatment, 1262 of these being alcoholic. That gives an average of 420 cases of that description admitted to and in some way disposed of every year. Of the 1262 alcoholic cases referred to, it was found necessary to send 115 to asylums for the insane—63 of these being males and 52 females. That shows an average of about 40 persons annually certified as insane and passed on from Ward VI. to asylums—the cause of the insanity in nearly all these cases being admittedly due to the drinking habit.

Looking closer at these figures, he found that in his experience of each of these 115 cases he had not been able to procure certification in lunacy by the medical men sent to the ward for that purpose until the patients had reached that stage of the disease in which the pathological conditions referred to by Dr Clouston were unmistakable. Medical men, looking to the present state of the law, and having a reasonable regard to their own safety and protection, declined to certify insanity at an earlier stage in the absence of these pronounced conditions.

It is painfully interesting to notice that the average age of those 115 drinkers sent to asylums is under 37 years; and that the time taken to acquire the necessary pathological qualifications for being certified as fit for the madhouse varied between ten and fifteen years, this difference no doubt depending upon the constitutional

susceptibility in each individual to the toxic action of alcohol in bringing about the necessary degree of brain destruction. No doubt the kind and amount of drink taken also bore some relation to the period of morbid development. It will be apparent from these considerations that before the habitual drunkard can be finally disposed of by transference to an asylum, a period of ten to fifteen years' active drinking will have to be undergone in order to bring the drinker within the scope of the law as it now stands. It is, of course, during this protracted period of inveterate drinking, when nothing by means of restraint can be done, that the domestic and social inconvenience and mischief are most oppressively felt. It may assist one to more adequately realize the magnitude and urgency of the matter under consideration to know that, in so limited an area as Ward VI., there are *annually* under treatment considerably over 400 of the class of patients described—a number equal to two-thirds of all the patients in the Royal Infirmary on any given *day* of the year, and that nearly one in every ten of these will be sent to asylums as incurably insane. These facts will doubtless impress you, as they have me, with the need which exists of obtaining legal control over these infatuated persons at a much earlier period, to be of any real service to them or to others interested in them. The insane qualification arrives too late. It is enormously destructive and fatal. It will doubtless also be evident that any shred of legislation intended for the benefit of a section of the community will be inadequate to meet the necessities of the situation. To do that any suitable measure, in order to meet with general public approval, ought to possess the recommendation of being adapted to meet the case of any individual in the community who may stand in need of the benefit of its application. In order to obtain the required legislation, our present notions as to the "freedom of the subject" may have to undergo some modification.

Mr W. C. Smith, advocate, said he thought it followed from the statements of Dr Connell, and from the experience in many other quarters, that most valuable results could be got from Dalrymple's Voluntary Retreat system, but then, as he understood the matter, the patients treated in Dr Connell's home were not the mentally diseased patients who were to be provided for in Mr Morton's Bill. He thought when they passed from the voluntary system to the principle of Mr Morton's Bill, that they arrived at a system which was impracticable and had no future. He thought the advocates of this system were in a dilemma. For political reasons they could not imprison a sane man who was drinking himself into insanity. They could not apply compulsion in the early stages of habitual drunkenness, which Dr Clouston had referred to. It was necessary before they could put such men into an asylum against their will, that it should be proved that mental disease was developed and established. He was afraid, when it was possible

to prove that in Court, all such cases were absolutely hopeless, and therefore the main reason for imprisonment disappeared. He thought the only way of dealing with the matter was by the use of moral influence to stop a man from drinking, and by reforming the licensing laws.

Mr J. P. Coldstream, W.S., said he had come here this evening and had listened with great interest to the discussion. They had heard in regard to this question connected with the measure of Mr Morton's that it would be exceedingly useful, and they had also heard that it probably would be detrimental, and it would be a bad measure to pass into law. He was a great admirer of Mr Morton, and anything proceeding from him would receive his careful consideration. He also was a great admirer of others who had spoken, and therefore he said he was left in a great dilemma if he was to go by medical men. What might be the result of this measure no one could tell. He certainly agreed with the sentiment which had been expressed, that they could not at this period of time legislate for one class only, and as he understood this Bill of Mr Morton's, it would apply to the upper class only, and not to the lower. He did not see why a Bill of this kind should not apply to the lower classes as well as to the upper class. If they were to be assessed for such patients, who to some extent must be considered as criminals, he said it was quite right that they should be assessed for supporting an institution such as had been proposed. He saw no objection on that score. He thought the Bill should be passed into law, but it should be one for all classes of society. The preamble had been objected to by Mr Shaw, and in regard to that matter he sympathized with the remarks which he had made. In regard to the period of detention, he should say two years was certainly a short enough time, but he should propose that it would be best not to limit the time, and detain the persons until they had a certificate of cure. He was glad to hear what Dr Connell had said, because up to that time it appeared to him that they had been labouring in the dark. He had given his experience. That experience was very valuable. He agreed that total abstinence was one of the best cures. Dr Connell had said that the work of his home was conducted on religious principles. That was in fact the best course. The spiritual remedy would in reality be the only radical cure of the disease from which these poor people suffered. They cannot always influence these people by religion, but in such cases as Dr Connell mentioned, the religious element being there was one of the utmost importance.

Professor Simpson said, that amidst all the differences of opinion, he thought there must be a wish that something should be done. Discussion would have great value in directing their minds to the need of legislative measures for the management of habitual drunkards,

and in showing the importance of some of those that had been advanced in Mr Morton's proposed Bill. But it seemed to him that the discussions would not have the full effect unless they were embodied in a resolution. He had been thinking of framing such a resolution, when their energetic Secretary put a draft resolution into his hands. This, with Dr Ritchie's approval, he had slightly modified, and would at once submit to the meeting as follows:—"That the Society memorialize the Secretary of State for Scotland and the Lord Advocate, praying the Government to initiate legislation for Scotland in the ensuing session of Parliament on the lines indicated in Mr Charles Morton's Draft Restorative Homes Bill, or in other ways that may seem to them in their wisdom to be more desirable, to provide compulsory powers of control, and detention of habitual drunkards in properly regulated houses."

Dr Littlejohn seconded, and the motion was unanimously agreed to.

Meeting VII.—March 6, 1888.

Professor CHIENE, *Vice-President, in the Chair.*

I. ELECTION OF NEW MEMBERS.

The following gentlemen were elected Ordinary Members of the Society:—John Berry Haycraft, M.D., D.Sc., F.R.S.E., 20 Ann Street; Henry Harvey Littlejohn, M.B., C.M., B.Sc., 24 Royal Circus; Albert Edward Morison, M.B., C.M., Hartlepool; William George Sym, M.B., C.M., 58 Queen Street.

II. EXHIBITION OF PATIENTS.

1. *Dr W. A. Finlay* showed a boy, æt. 12 years, on whose left foot he had performed MICKULICZ'S OPERATION. It was done for disease of the tarsus, and the result was a success. He was debarred from performing Syme's amputation by the presence of an ulcer on what would have been the heel flap. The operation was performed seven months and a half ago, on 16th July 1888. The union of the bones is now firm, and the boy can walk well and with ease. The limb operated on is half an inch longer than the other, measuring to the heel in the right leg and to the ball of the great toe in the left.

2. *Mr A. G. Miller* showed a case of FRACTURE OF THE BASE OF THE SKULL, and read the following notes:—

Case of Fracture of Base of Skull.—A H., female, æt. 19. Admitted to Ward XVII. on 6th Feb. 1889.

History, obtained from others, for patient has no recollection of accident. Patient was hanging out clothes at window when the pole on which she was leaning broke, and she fell a distance of three stories.

On admission patient was insensible, and suffering from condition of so-called concussion. There was a scalp wound, about 2 inches long, on *left* side posteriorly. There was bleeding from the right ear, and ecchymosis of right eyelid. No subconjunctival hæmorrhage. (Temperature was persistently subnormal.)

Diagnosis.—Fracture of base of skull oblique from behind forwards towards the right.

On the day after admission patient's respiration became sighing. On suspicion that the origin of the vagus nerve might be irritated the urine was examined, and the usual reactions indicating the presence of sugar were noticed. The glycosuria and sighing respiration disappeared after twenty-four hours. Patient recovered rapidly.

3. *Dr James Carmichael* exhibited two patients who had recovered from PURULENT PLEURITIC EFFUSION, treated by aspiration.

CASE I.—That of a girl, aged 8, who was admitted to the Royal Hospital for Sick Children 24th October 1888, and dismissed 14th January 1889. On admission the signs of fluid at the base of the right chest were well marked. She weighed 34 lbs. on admission, and 41½ lbs. on dismissal. Two days after examination five ounces of thick creamy pus were drawn off from the posterior part of the chest below the scapular angle. She experienced some, but only partial, relief, and for several days was much troubled with paroxysmal cough. On the 30th inst., or four days after tapping, she expectorated a quantity of muco-purulent matter, with manifest alleviation to her symptoms. The chest was subsequently explored with the hypodermic needle at several points, and no pus found. She did not improve during the next fortnight, the symptoms pointing to reaccumulation of fluid in the pleura. Exploration failing to find fluid anywhere posteriorly, the needle was introduced anteriorly in the fourth intercostal space and pus found, about 2 ounces being drawn off. She still expectorated at intervals small quantities of muco-purulent matter during the paroxysmal fits of coughing. The subsequent progress of the case showed gradual and steady improvement as regards general symptoms and physical signs, and she may now be seen to have made a perfect recovery. The diameter of her right chest is two-thirds of an inch less than the left.

CASE II.—A boy, aged 8, admitted 28th November, dismissed 14th January. He had been subject to bronchial cold in the winter for several years, but was otherwise well, not having suffered from any of the acute diseases. A fortnight before admission he returned home from school soaking wet, and thereafter

suffered from cough and debility. On admission he was ill nourished, and weighed 38 lbs. There were well-marked signs of limited effusion quite at the base of the right lung posteriorly, and on introducing a needle some thick curdy-looking pus was drawn off. He suffered from slight cough and some sweating at night, but was otherwise well. He was put on maltine with cod-liver oil and generous diet. On 4th December the physical signs having only slightly improved, a needle was introduced in the same situation, and a similar quantity of pus came away. A week afterwards exploration was tried, and no pus discovered. From this time the physical signs steadily improved, and the patient was dismissed, having increased in weight 4 lbs.

These two cases, it appears to me, are of interest, not in showing anything new in the clinical features of this disease, but in demonstrating the fact that purulent collections in the pleura can be got rid of effectually, although exceptionally, by aspiration alone. In Case I. the occasional complication of pleural perforation and spontaneous discharge of pus per orem is demonstrated, and it may be questioned whether in the absence of Nature's assistance in this way, aspiration would have been successful. Such spontaneous discharges of pus in empyema are not in my experience very uncommon. As Trousseau long ago pointed out, the occurrence is generally due to ulceration of the pleura over small localized pneumonic areas. In this case also the cavity was bilocular.

III. Exhibition of Specimens.

1. *Mr A. G. Miller* showed—

(*a.*) A CAST of a dislocated knee-joint and parts removed by operation. The patient from whom these were taken was a male, æt. 19, from Aberdeen.

Admitted to Ward XVIII., 21st February 1889.

History.—Four years ago patient, when jumping into a cart, struck his left knee against the side of the cart. The limb at once assumed the appearance it presented on admission. Patient was able to walk and bear his weight on limb. Very little seems to have been attempted in the way of treatment for two years, during which time the lad was able to do work. During the last two years several attempts have been made to improve the form and mobility of the joint.

On Admission.—The left knee-joint is flexed and almost completely anchylosed. There is some swelling and pain on movement. There is a displacement of the tibia outwards, with outward rotation, so that the fibula lies behind. The patella is fixed.

Diagnosis.—Old dislocation of tibia outwards, with possibly fracture of the external condyle of femur. Any attempt at reduction being, of course, out of the question, excision of the joint was advised, and agreed to by the patient.

On 23rd February the knee-joint was excised, when the dislocation was made very plain, and evidence of commencing tubercular disease discovered. There is a mark on the external condyle, which looks very like a partial fracture.

(*b.*) A PORTION OF INVAGINATED BOWEL removed after two days' obstruction in a gangrenous condition. This was from a boy æt. 12.

Admitted 27th February 1889.

History.—On 25th, patient was preparing to go to school when he was suddenly seized with pain in the abdomen on the right side, immediately after which he passed a loose motion in the w.-c. (patient cannot say whether there was any blood.) There has been no motion of the bowels since. Patient then began to vomit. Vomiting became fæcal on second day, and continued up to time of admission. He has had an anxious expression ever since illness commenced.

On Admission.—Abdomen moderately distended and slightly tender. Tympanitis not marked. A firm elongated mass is easily felt in right iliac fossa extending upwards into the lumbar region, and about four or five inches in length. Nothing abnormal felt by rectum. Patient very weak and collapsed.

Diagnosis.—Intussusception into ascending colon. Immediate operation was recommended, and at once agreed to by parents, who accompanied patient.

On opening abdomen intussusception was at once discovered. Attempts at undoing the invagination failed completely. The portion of invaginated bowel was at once excised and removed. As the patient was in a very feeble condition, the bowel openings were secured to the external opening in the abdominal walls for the formation of a fæcal fistula. Patient sank, and died in the course of the evening.

A very fine cast by Mr Cathcart showed the condition of parts when removed.

2. *Mr Cathcart* showed a BLADDER taken that day from the body of an old woman who was admitted in a moribund state to Mr Miller's ward, supposed to be suffering from a fracture of the neck of the femur, which it was found, however, that she did not have. At the post-mortem the interior of the bladder was found to be simply a clot of blood. Her only bladder symptom during life was incontinence of urine. She had not passed any blood. It was evident that there was an infiltration of blood into the walls of that viscus. The explanation of this remarkable condition was not clear.

IV. Original Communication.

ON THE ONLY WAY OF RAISING THE TONGUE, THE EPIGLOTTIS, AND THE ARYTENO-EPIGLOTTIC FOLDS AT ONE AND THE SAME TIME.

By James Foulis, M.D., C.M., F.R.C.P. Ed.

Mr President and Gentlemen,—The object of the present paper is twofold. First, to demonstrate what are the natural bodily structures which occasionally act as valvular impediments to the passage of air into the lungs; and, secondly, to show that there is but one safe and thoroughly reliable way of removing the obstruction so produced in all cases of apnœa, whether occurring in poisoning, in chloroform or opium narcosis, etc., or in the apparently drowned.

Let me ask you now to perform the following experiment:—Place two fingers on the upper border of the hyoid bone, in the middle line, and then press with the finger-tips directly backwards, and at the same time try to draw in your breath. You will find that a very slight amount of backward pressure is sufficient to cause complete obstruction to the entrance of air into the lungs through the glottis. Indeed, you cannot possibly draw in your breath if you press backwards with your finger-tips in the way I have directed. Something seems to act as a valve, and it entirely prevents air passing downwards at this spot when the attempt is made to draw in the breath. The obstruction is not at the rima glottidis. It is much higher up. On removing the pressure you can draw in your breath with ease, but on reapplying the pressure the valve again acts, and cuts off the ingress of air. What is this something? It is the epiglottis.

If you look at this vertical mesial section of the head and neck, kindly lent to me for the purpose of demonstration by Dr Symington, you will see when I press back with the finger-tip at the spot, as you have just been doing, that the part which first comes in contact with the posterior pharyngeal wall is the epiglottis. When the finger-tips press backwards, they first of all push back the base of the tongue, which is attached to the hyoid bone, and the base of the tongue pushes back the epiglottis until the latter comes in contact with the posterior pharyngeal wall, against which it is flattened, and when an attempt is made to draw in the breath it acts as a valve and entirely prevents air passing.

On examining carefully the preparation, you will see that the epiglottis lies immediately behind and in contact with the hyoid bone and the base of the tongue, to both of which structures it is closely attached by ligaments, and that above the level of the upper border of the hyoid bone the epiglottis projects quite free for at least half an inch. It is this free edge of the epiglottis

which is pushed back against the posterior pharyngeal wall when the fingers are made to press backwards as I have directed.

Now, in this case it is right to notice that the epiglottis does not fall down over the entrance into the glottis and thus prevent the ingress of air. It acts as a valve when it is pushed back, or when it falls back, and is flattened against the mucous membrane of the pharyngeal wall, the glottis being quite free. If, when the epiglottis is thus flattened against the pharyngeal wall, a strong expiratory effort is made, the free edge of the epiglottis can be made to vibrate coarsely; but any attempt to draw in the breath will fail as long as the epiglottis is thus flattened against the pharyngeal wall.

It is very important to observe also, while the fingers are thus pressing back the epiglottis and causing an obstruction to the ingress of air, that if the tongue is darted forward or forcibly protruded, the obstruction to the breathing at once disappears; and that as long as the tongue is thus forcibly protruded, backward pressure with the finger-tips quite fails to obstruct the breathing, even when the person experimented on is lying on his back.

The explanation of all this is easily given. In the first place, we must note that the natural position of the epiglottis is such that a very slight amount of backward pressure in the middle line is sufficient to flatten it against the posterior pharyngeal wall. In the natural position it must lie close to the pharyngeal wall, judging from the very short distance necessary to press it backwards with the finger-tips in order to cause complete obstruction to the breathing. The most important fact to remember is that the epiglottis is so closely and firmly attached both to the hyoid bone and to the base of the tongue, that the base of the tongue and the hyoid bone cannot be moved forward or away from the pharyngeal wall without dragging on the epiglottis.

When the tongue is forcibly protruded, the muscles which pass between the lower jaw and the hyoid bone, with the genio-hyoglossi muscles, so pull on the base of the tongue and the hyoid bone that the epiglottis is forcibly drawn away when these parts move forward. Hence, in our present experiment, although we can cause complete obstruction to the ingress of air by pressing back the base of the tongue and the epiglottis against the pharyngeal wall, this obstruction can be immediately overcome by darting forward the tongue, and as long as the tongue is forcibly protruded we cannot press back the epiglottis against the pharyngeal wall. If, while the finger-tips are resting on the tuberosity of the hyoid bone in the middle line, the tongue is forcibly protruded, the hyoid bone will be found to slip past the finger-tips. This movement of the hyoid bone tells us that both the base of the tongue and the epiglottis are at the same time drawn forward away from the pharyngeal wall.

These simple experiments teach us that if the base of the tongue

and the hyoid bone can be drawn forwards, either by muscular action or by other means, it is quite impossible for the epiglottis to fall against the pharyngeal wall or in any way obstruct the passage of air into the lungs during inspiratory efforts.

If, however, the tongue and the hyoid bone are allowed to fall backwards as the result of loss of muscular tone, as in anæsthesia, or by mechanical pressure, there is always the great danger that the epiglottis will become flattened against the pharyngeal wall and thus obstruct the breathing.

Let me now show you two other preparations, which will, I think, demonstrate conclusively that in opium or chloroform narcosis, and in the apparently drowned, the mechanical obstruction to the breathing is caused by the tongue and the epiglottis lying against the posterior pharyngeal wall.

These two valuable preparations were lent to me for this demonstration by Dr Symington. The man from whom they were taken died as he lay on his back. As soon as possible after death the body was frozen, and sections were made through the mouth and pharyngeal region in the frozen condition, so as to catch the relation of the tongue and epiglottis to the posterior pharyngeal wall. These two sections demonstrate conclusively that when the muscles and tissues which attach the tongue and the hyoid bone to the lower jaw become relaxed, as in chloroform or opium narcosis, or in the apparently drowned, the tongue and the epiglottis will fall backwards until they come in contact with the mucous membrane of the pharyngeal wall. The thyrohyoid membrane is the hinge, as it were, on which the tongue and the hyoid bone *en masse* move backwards and forwards. The epiglottis lying immediately behind the base of the tongue and the hyoid bone, and being firmly attached to these parts, must necessarily move backwards and forwards with the hyoid bone and the base of the tongue, when the latter move in these directions.

Dr Symington thus describes these preparations:—

Fig. 1.—A horizontal section of the head of an adult male subject, as seen from above. The body was frozen lying on its back. The section passed through the mouth at the level of the edge of the lower lip and the upper parts of the crowns of the teeth of the lower jaw (*a*). Farther back, the vertebral column was divided opposite the body of the axis vertebra (*f*). The mucous membrane of the dorsum of the tongue was cut along a transverse line, at the level of the apex of the V-shaped group of circumvallate papillæ (*d*). For about half an inch on either side of the middle line this part of the tongue was separated from the posterior wall of the pharynx by an interval of about 3 mm. Farther outwards, the mucous membrane of the tongue was in contact with the tonsils (*k*). The cavity behind the tongue formed a curved slit with its concavity forwards, and about two inches in length.

Fig. 2.—A horizontal section $\frac{3}{4}$ of an inch below No. 1. This is

FIG. I.

a. Incisor teeth.
b. Tongue.
c. Lower jaw.
d. Mucous membrane of tongue.
k. Tonsil.
h. Mucous membrane of post. pharyngeal wall.
f. Body of axis vertebra.
g. Spinal chord.

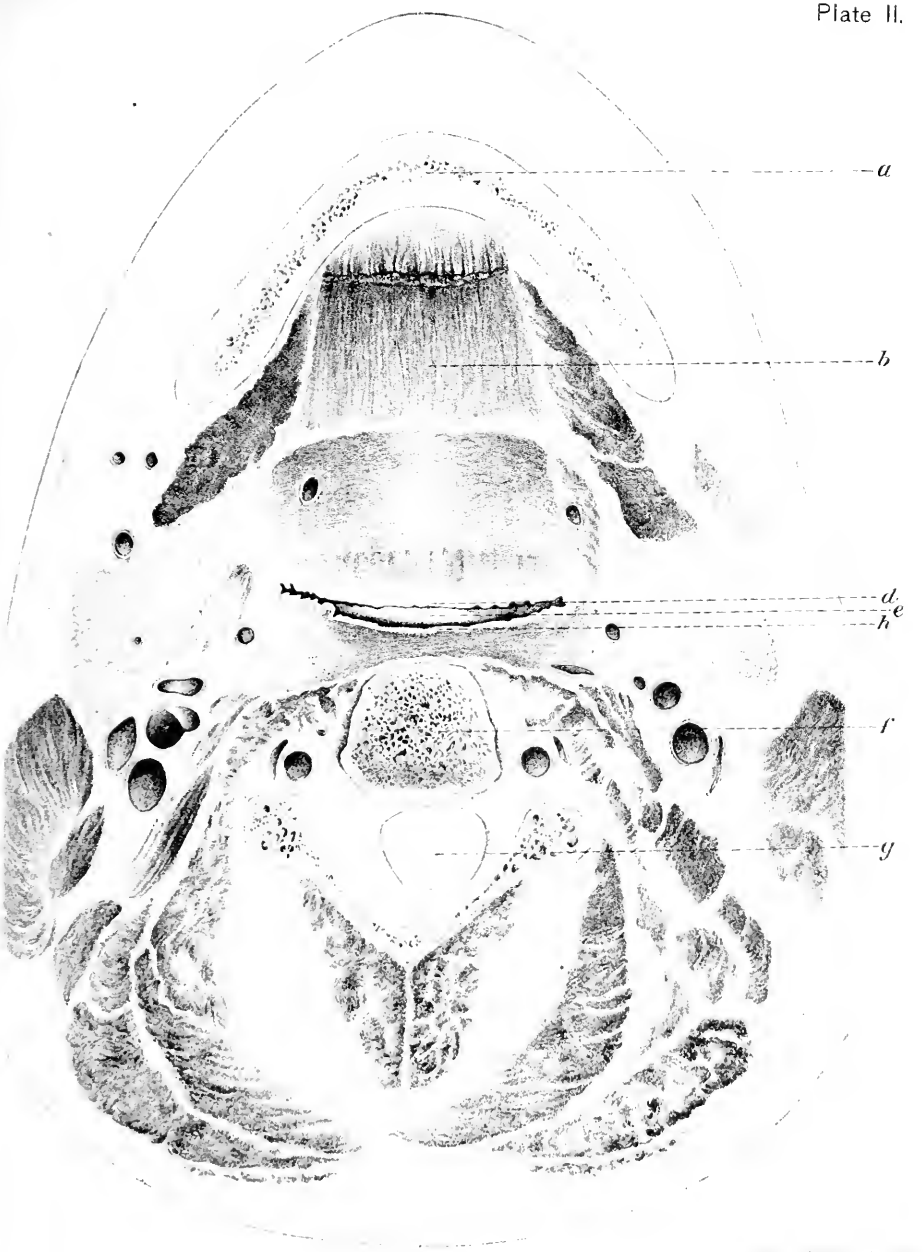


FIG. 2.

a. Lower jaw.
b. Tongue.
d. Mucous membrane of tongue.
e. Epiglottis.
h. Mucous membrane of post pharyngeal wall.
f. Body of 3rd cervical vertebra.
g. Spinal chord

also looked at from above. This section is through the body of the lower jaw (*a*) and the third cervical vertebra (*f*). The pharynx is divided immediately above the epiglottis. The pharynx formed a transverse slit about $1\frac{1}{2}$ inches in length, and was bounded in front by the mucous membrane of the tongue (*d*). Just below the plane of this section was seen the epiglottis (*e*), which projected upwards into the pharynx with its anterior surface in contact with the mucous membrane of the tongue, and its posterior against the posterior pharyngeal wall (*h*). The epiglottis was in contact with the posterior wall of the pharynx for fully an inch below the plane of this section.

In the first preparation attention is drawn to the close relationship of the tongue to the posterior pharyngeal wall at the situation of the V-shaped circumvallate papillæ. At this spot, for about half an inch on either side of the middle line, this part of the tongue was separated from the posterior pharyngeal wall by an interval of about 3 mm.; and farther outwards, on the same level, the mucous membrane of the tongue was in contact with the tonsils.

In the second preparation, which was made about three-quarters of an inch below No. 1, we find the pharynx was divided immediately above the tip of the epiglottis, which can be well seen in the drawing lying between the tongue and the posterior pharyngeal wall, and in close contact with both these parts. The epiglottis evidently has been pressed back against the mucous membrane of the pharyngeal wall, and lies flattened against it; and the remarkable fact comes out, on careful examination of this preparation, that the epiglottis was in contact with the posterior wall of the pharynx for fully an inch below the plane of the section. I think, then, we may conclude there is every probability that these sections represent very closely the relation of the epiglottis and the tongue to the posterior pharyngeal wall in all cases where the breathing has become obstructed in consequence of the tongue falling backwards as the result of relaxation of its muscular supports.

Granting, then, that the natural bodily structures which may seriously interfere with respiration when a patient is insensible, or under the influence of a poison or of an ordinary anæsthetic, are the tongue, the epiglottis, and the aryteno-epiglottic membranes, it is our object, in the next place, to show how these parts can be safely and completely removed in all cases of apnœa, and especially in those cases where artificial respiration becomes necessary as a means of reviving the patient.

With regard to any obstruction within the larynx itself there is no evidence to show that the vocal chords seriously interfere with the passage of air into the lungs during the administration of an anæsthetic. One occasionally hears a peculiar laryngeal sound as if the vocal chords were in a state of tension, and were offering

some resistance to the passage of air inwards; but it is very certain that if this noise results from muscular spasm within the larynx, the continued administration of the anæsthetic will quickly cause it to disappear. Not so, however, with the obstruction which occurs as the result of the tongue and epiglottis valving the air passage. The deeper the anæsthesia in this case the greater the danger of the obstruction, especially when the patient is lying on his back.

One of the first well-marked effects of chloroform anæsthesia is relaxation of muscles in all parts of the body.

When a patient is lying on his back in a state of chloroform narcosis, those muscles which attach the tongue to the lower jaw, to the hyoid bone, and to the styloid processes become relaxed, and from this cause the tongue with the hyoid bone, moving on the thyrohyoid membrane as a hinge, has a tendency to fall backwards and to come in contact with the pharyngeal wall, and during respiratory efforts the tongue, as a large ball-like flabby mass, may be sucked downwards till it presses against the posterior pharyngeal wall, in which position it, with the epiglottis, effectually prevents all inspiratory efforts.

Now, it is in the experience of all Edinburgh surgeons that, when the tongue has thus fallen back and the patient is in danger of suffocation, instant relief may be often given to the patient's breathing by dragging forcibly on the tongue with a pair of catch forceps. The method which seems to answer best is to drag the tongue forcibly *upwards* and forwards until the anterior fibres of the geniohyoglossi muscles are put on the stretch. To make the method effectual, the tongue must be dragged forwards with considerable violence. In this way the body of the tongue is drawn away from its contact with the pharyngeal mucous membrane, and allows the epiglottis to rise up by its own elasticity sufficiently to allow air to pass between it and the pharyngeal wall during inspiratory efforts. In the great majority of cases this method succeeds in relieving obstructed breathing; but all of us have seen cases in which this dragging on the tip of the tongue did not effectually relieve the obstruction. And the question has frequently been asked, Does this dragging on the tip of the tongue also drag on the hyoid bone and the epiglottis? If dragging on the tip of the tongue did draw away the hyoid bone and the epiglottis from the pharyngeal mucous membrane, we should have a certain method of relieving the obstruction in all cases where the tongue and the epiglottis were the cause of the obstruction; but experiments on the dead body have proved that dragging forcibly on the tip of the tongue does not drag upon either the hyoid bone or the epiglottis. It only stretches the fibres of the tongue until the anterior muscular fibres of the geniohyoglossi muscles and fibres of the styloglossi muscles are put on the stretch. Experiment has also shown that, before the hyoid bone and the epiglottis can

be dragged upon in this way, the fibres of these muscles must be actually torn through. Dr Benjamin Howard, of New York, ten years ago demonstrated all this; and eight or nine years ago I also demonstrated these facts to this Society by means of numerous wet preparations, which were lent to me for the occasion by Professor Turner. If you now look at this preparation, which is a vertical mesial section through the head, neck, tongue, hyoid bone, and epiglottis, you will find that when I drag the tongue forcibly upwards and forwards, I simply drag the tongue away from the pharyngeal wall until the anterior fibres of the geniohyoglossi muscles and the fibres of the styloglossi muscles are put on the stretch, and there I must stop, or I shall tear through those muscles. You will see that, though I am dragging forcibly, the hyoid bone and the epiglottis are quite unaffected; but I wish to draw your attention to one important point, and it is this—if I now continue to drag forcibly upwards, you will see that the lower jaw is drawn upwards, and as a consequence of this upward movement of the lower jaw the muscles which pass between it and the hyoid bone are put on the stretch, and the hyoid bone is now pulled a little upwards, and the epiglottis moves with the hyoid bone, but the distance moved by the hyoid bone and the epiglottis is very slight.

We may then take it as a fact that dragging forcibly on the tip of the tongue with a pair of catch forceps cannot directly affect the hyoid bone or the epiglottis, except through the medium of those muscles which pass from the lower jaw to the hyoid bone.

I do not suppose there is any one present to-night who has not seen the good effect of dragging on the tongue in cases of obstructed breathing during chloroform administration. How then does this method act if dragging on the tip of the tongue does not drag either on the hyoid bone or on the epiglottis? It acts simply by stretching the fibres of the tongue in such a way that the tongue is drawn well away from the pharyngeal wall, and allows the epiglottis by its own elasticity to rise up from its contact with the pharyngeal wall sufficiently to allow air to pass downward during inspiration; but there is always this danger in the method—even though the tongue may be drawn quite away from the pharyngeal wall, and the epiglottis be not in actual contact with the mucous membrane of the pharyngeal wall, yet the epiglottis may lie so close to it from complete relaxation of the muscles which pass between the lower jaw and the hyoid bone, that there is always a danger of its acting as a valve during the inrush of air in inspiratory efforts. This is what undoubtedly occurs in those cases in which we see that dragging on the tongue more or less fails to give complete relief to the breathing. It cannot be too strongly insisted upon, that in such cases of obstructed breathing the obstruction is not caused by the epiglottis falling down on top of the glottis and thus closing the chink of the glottis.

If the muscles which pass from the lower jaw to the hyoid bone are quite relaxed, the base of the tongue, the hyoid bone, and the epiglottis may fall back against the posterior pharyngeal wall; but the chink of the glottis is not actually closed by the epiglottis falling upon it.

The air-passage, which naturally lies between the epiglottis and the pharyngeal wall, is a long narrow slit, and a very slight pressure by the finger-tips in the way I have described flattens the expanded epiglottis against the posterior pharyngeal wall. In the condition of relaxed muscular supports, as in deep anæsthesia, the very act of breathing inwards may cause the epiglottis to valve against the pharyngeal mucous membrane; and if it is true that dragging forcibly on the tip of the tongue cannot directly affect either the epiglottis or the hyoid bone, we are forced to the conclusion that this method of dragging on the tip of the tongue is not one that we can always depend upon, and therefore we must look for another method which will enable us, at one and the same time, not only to raise up the tongue from its contact with the posterior pharyngeal wall, but also to raise the epiglottis, and render tense the aryteno-epiglottic folds; for unless this is done, especially in cases where artificial respiration is necessary, the passage of air in and out of the lungs will not be free and unobstructed.

It was Sir Joseph Lister who taught us that forcibly dragging on the tip of the tongue relieved the breathing in a reflex manner. As I understood it, his theory was this—that the powerful stimulus of traction on the tip of the tongue set up in certain muscles a contraction, and this contraction acting on certain obstructing parts drew them away out of the road of the inrushing air. It is difficult to see how reflex action can in any way be brought into play in deep anæsthesia, and the deeper the anæsthesia the weaker is the reflex action; but I can quite believe that in the early stages of anæsthesia strong traction on the tip of the tongue may set up in a reflex manner a contraction in those muscles which pass between the lower jaw and the hyoid bone; and, as we know, if the hyoid bone is drawn away by this muscular contraction, the epiglottis will go with it. In the same way the hyoglossi muscles might be stimulated to contract, but there comes a time in deep anæsthesia when all reflex action is abolished, in which condition the stimulus of powerful traction will be of no avail.

My friend Dr John Duncan has suggested to me that this strong traction on the tip of the tongue acts mechanically on the hyoid bone and the epiglottis—that is to say, the strong traction necessary to put the anterior fibres of the geniohyoglossi muscles on the stretch, if continued, will draw up the lower jaw, and at the same time draw on the hyoid bone through the medium of the muscles which pass between the lower jaw and the hyoid bone.

For years past it has been my custom when giving anæsthetics,

in cases of obstructed breathing, not only to push the jaw forward at its angles, but also to draw the lower jaw well forward by seizing it at the symphysis, which may be easily done by hooking a forefinger behind the front incisor teeth, and placing the thumb below the jaw at the symphysis. In this way, of course, traction acts on the hyoid bone through the muscles attached to it. In all our methods of relieving obstructed breathing, we must not forget that whatever acts on the hyoid bone acts similarly on the epiglottis.

The method of relieving obstructed breathing occurring during the administration of anæsthetics, by dragging forcibly upwards and forwards on the tip of the tongue, is now so well established in our hospital here, that it will be difficult to displace it by another method, even though it can be shown how a new one acts, and that it is a perfectly reliable one under all circumstances.

That dragging forcibly on the tip of the tongue is a valuable means of relieving obstructed breathing all of us are forced to admit; but few of us would assert that it is a perfectly reliable method in all cases, or deny that it is a very cruel method, especially in the case of young people and children.

We may have some doubts as to the effect on the epiglottis and the hyoid bone of forcible traction on the tip of the tongue with catch forceps; but there can be no doubt as to the effect on the epiglottis if we draw forward the base of the tongue and the hyoid bone.

Many of our leading surgeons years ago taught that hooking forward the base of the tongue and the hyoid bone with the forefinger was a reliable method of relieving obstructed breathing. The late Professor Spence taught this method to his class—any method by which the hyoid bone and the base of the tongue can be hooked forward will at once draw away the epiglottis from its contact with the pharyngeal wall.

The latest attempt to abolish the cruel method of forcibly dragging on the tip of the tongue with catch forceps is that which was embodied in a paper by Dr Benjamin Howard of New York, entitled, "A New and Only Way of Raising the Epiglottis." This paper was read before the Medical Society of London on the 20th of October 1888. Dr Howard undoubtedly, as the result of his anatomical investigations, saw that if the hyoid bone could be drawn forward, either by muscular or mechanical means, the epiglottis will move with it, and he has proposed the following plan as the best means of drawing away the epiglottis and of thus relieving the obstruction to breathing in a patient under the influence of an anæsthetic:—One hand is placed under the chin and another on the vertex of the skull, and the head and neck are now to be bent back until the spines of the vertebræ lock against each other and the skin in the middle line of the neck is rendered

tense. When this has taken place, the muscles which pass between the lower jaw and the hyoid bone (the elevators of the hyoid bone) are so put on the stretch that the hyoid bone will be drawn well forward, and with it will move the epiglottis. The key to the situation is the hyoid bone, as Dr Howard has remarked. Now the object of this particular method is to draw away the epiglottis from the pharyngeal wall and to open up the post-oral breathing channel,—that is to say, the breathing must, in this method, take place through the nares, as the mouth is closed by the upward pressure of the hand which is placed below the jaw. The fact that the mouth is closed in this method is enough to condemn it, as we could not trust to it in cases of artificial respiration, where everything depends on the rapidity with which air is pumped in and out of the lungs. As we all know, the nares are often obstructed with dirt and disease, and when the mouth is closed the free nasal flaps may act as valves during forced inspiratory efforts.

That this extreme extension of the head and neck, until the skin of the neck in the middle line is stretched, will render tense the muscles which elevate the hyoid bone no one will deny; but there are very grave doubts whether at the same time the large flabby tongue will be drawn away from its contact with the posterior pharyngeal wall even though the epiglottis itself may be drawn away.

If you turn to the *British Medical Journal* for 11th June 1888, you will find that Dr Jamieson of the Indian Medical Service has recorded a case in which Howard's method of head and neck extension failed to move the tongue away from its contact with the pharyngeal wall. Here is what Dr Jamieson said:—"The case was that of an old Hindu woman, aged about 50, who insisted on having chloroform for a slight operation. Heart and lungs revealed nothing abnormal on stethoscopic examination. The chloroform was administered by a senior student by means of a Junker's inhaler. After about one minute, respiration suddenly ceased, and I at once tried, by means of extending the head and neck to their full extent, to free the respiratory passages. This I found I could not do, for, on applying artificial respiration, air neither passed in nor out of the lungs. I at once passed my finger into the mouth, and found the tongue fixed at the back of the pharynx. The tongue was drawn forward, held with forceps, and artificial respiration resumed. Now I was at once enabled to pass air freely in and out of the lungs, and, after some five or ten minutes, had the satisfaction of seeing my patient recover."

Dr Howard, in reply to Dr Jamieson, thus writes in the *British Medical Journal* for 2nd February 1889:—"I am indebted to a recent correspondent of the *Journal* for the opportunity of adding to the prominence of a single point I had failed, perhaps, to make sufficiently conspicuous. Whereas by head and neck extension

the raising of the epiglottis cannot be missed, the post-oral air-way is most easy altogether to fall short of. In every effort I have witnessed to do my method, as was thought, the first time, this is just what has happened: From an apparent fear of doing harm that part of the extension I have described by the word 'utmost' has been stopped just short of, whereas this is just the part of it which, to secure the post-oral air-way, is indispensable. Even this will be ineffectual, unless with it is combined a perpendicular pharynx and a horizontal os palati. To dispel the fear of harm it should be understood that there is a definite and insurmountable safety check. This consists in the firm wedging together of the cervical spinous processes, which form a solid and unbreakable arch."

So that, according to Dr Howard, everybody who has tried his method for the first time has failed; and there is likely to be failure again, unless extension of the head and neck is made to the "utmost." That is to say, to make the method successful in every case, the neck must be bent back until there is a "firm wedging together of the cervical spinous processes, which form a solid and unbreakable arch." "Even this will be ineffectual, unless with it is combined a perpendicular pharynx and a horizontal os palati."

We may be sure that no method of raising the tongue and the epiglottis which demands so much to make it successful can ever become popular. There are very few men who may be called upon to try Howard's method for the first time who will have the courage to bend back the neck to the utmost. And even though this is done, how can we be quite certain that the tongue is at the same time drawn well away from the pharyngeal wall, without which the post-oral air-way cannot be free? Is there "no fear of harm" in thus bending back the neck to the "utmost" in young persons and in children?

It is the "fear of harm" that will make men at all times hesitate to bend back the neck to the "utmost." And lastly, in cases of artificial respiration, where it is sometimes necessary for many hours at a time to keep open the air passage into the lungs, it would be a very difficult thing to keep up this "utmost" extension of the neck for such a long period of time.

In connexion with this subject it is right to mention another source of danger. Your President, Dr John Smith, many years ago pointed out that in such extreme bending back of the head the internal jugular veins may be dangerously compressed against the transverse processes of the atlas vertebra.

Enough has been said to show that Howard's method of extension of the neck, while it undoubtedly renders tense the elevators of the hyoid bone, and in this way raises the epiglottis from the pharyngeal wall, is not one that can at all times be relied upon as a means of raising the tongue from its contact with the

pharyngeal wall; and if the tongue is not kept free from contact with the posterior pharyngeal wall, the post-oral air-way cannot be free.

Now what we require, and what we are looking for, is a simple means by which we can with certainty, under all conditions of apnœa, raise up the whole tongue and the epiglottis from contact with the posterior pharyngeal wall; but not only so, the method must be one which will enable us to keep these parts raised up from contact with the posterior pharyngeal wall, say for hours at a time if need be, during artificial respiration. For it must be clear to all that the act of raising up the tongue and the epiglottis in these cases of obstructed breathing may not be enough. In the great majority of such cases we may be called upon to perform artificial respiration after we have raised up the tongue and the epiglottis; and therefore it is necessary that our method should be one that not only effectually raises the tongue and the epiglottis, but also makes it absolutely certain that these parts shall not fall back, or be sucked downwards by the rush of air into the chest during the movements of artificial respiration.

It is not enough simply to make tense the elevator muscles of the hyoid bone, and to raise the epiglottis, either by Howard's method, or by pushing or pulling forward the lower jaw; for in none of these methods can we be sure that the large, flabby tongue is at the same time drawn away from all danger of contact with the pharyngeal wall. Nor is it enough to draw the tongue violently forwards and upwards, for, as has already been pointed out, though this method undoubtedly stretches the tongue to its fullest extent, and draws it well away from contact with the pharyngeal wall, experiment has proved that this method cannot drag upon the epiglottis, except through the medium of the muscles which pass between the lower jaw and the hyoid bone, and that such an amount of force as is necessary in this way to drag forward the tongue and the epiglottis at one and the same time, by means of the catch forceps, is hardly justifiable. Besides, one would hardly dare to drag with such force on these structures for any lengthened period in artificial respiration; and in the case of infants and young children it would be quite unjustifiable to do so.

Now, the method of raising the tongue, and of raising the epiglottis and stretching the aryteno-epiglottic folds at one and the same time, which for some years past I have advocated, is one that enables us to give instant relief in all forms of obstructed breathing, and in doing so, makes it absolutely certain that there is no risk of the tongue or the epiglottis being sucked downwards or valving against the pharyngeal wall during the movements of artificial respiration.

Bearing in mind the anatomical facts to which I have already referred, namely, that the base of the tongue is so intimately connected with the hyoid bone and the epiglottis that if it, the base,

can be tilted or hooked forward, the hyoid bone and the epiglottis will at the same time go forward with it; we then see that if we can pass an instrument, such as our forefinger, down behind the tongue as far as its base, and hook forward the base, the hyoid bone will be carried forward, and at the same time the epiglottis will be raised and the aryteno-epiglottic folds put on the stretch; and as long as such an instrument is in this position, one can keep the body of the tongue and the epiglottis so far removed from the pharyngeal wall that there is not the least danger of these parts acting as valves, or in any way obstructing the ingress of air during the movements of artificial respiration.

The forefinger would be the best of all instruments for this purpose, but there are several objections to it. In the first place, it is sometimes almost impossible to get the forefinger into a patient's mouth owing to the rigidity of the jaws, and there is always the risk of being severely bitten by the half conscious patient. The tongue, too, is a large, slippery, flabby mass, and very difficult to move by means of the forefinger in consequence of its slippery nature. The finger also takes up too much room in the mouth, and may not be sufficiently long to reach to the base of the tongue.

It will be in the recollection of many members of this Society, that early in the year 1880 I read before this Society a paper entitled "The Treatment of Asphyxia during the Administration of Anæsthetics." In that paper I recorded a case of opium poisoning, in the treatment of which it was necessary to carry out artificial respiration for many hours under very trying circumstances. The case, shortly, was as follows:—Late one night I was sent for hurriedly to see a lady who was said to be either dead or dying. When I arrived at the house, at two o'clock in the morning, I found the patient in bed lying on her back and apparently dead. I could get no satisfactory history of the case at the moment. She was pulseless at the wrist, and respiration had ceased. She was deeply cyanosed, and a cold, clammy moisture was on the body. With the stethoscope I detected evidence of slight cardiac action, which proved to me she was still alive. Looking into her eyes by means of a candle-light, I found the pupils contracted to a pin's point. I then threw back her head over a pillow and passed my finger down her throat as far as the base of the tongue, which I hooked forward. Now, mark what occurred immediately I hooked forward the base of the tongue. There was a slight inward rush of air. From this it was quite clear to me that at the end of an expiratory effort the tongue had fallen back, and at the moment of an inspiratory effort it had been sucked downwards and backwards against the pharyngeal wall, where it remained as a valve entirely preventing the passage inwards of air. On removing this valve the natural resilience of the chest wall came into play, and air rushed into the chest

through the glottis immediately the chest wall expanded. I soon came to the conclusion that the case was one of opium narcosis, and that artificial respiration must be energetically carried out. As soon as I took my finger out of the mouth, the tongue fell back again, and valved against the pharyngeal wall; and I then adopted a very simple means to prevent this occurring.

Howard's method of chest compression was carried out for twelve hours in this case, and during this time I proved to myself, over and over again, that neither it nor any method of artificial respiration can be of use in such cases as long as the tongue and the epiglottis are allowed to obstruct the patient's breathing. As the patient lay on her back in a deeply insensible state, two pillows were placed under her shoulders, and the head was allowed to fall well back. The handle of an ordinary tablespoon was then passed with the greatest ease down behind the patient's tongue as far as the base, and as the spoon was levered backwards the hyoid bone and the base of the tongue were made to project as a lump in the middle line of the patient's neck, and as long as this lump was seen in the patient's neck I felt quite certain that neither the epiglottis nor the tongue could in any way again obstruct the breathing during the process of artificial respiration.

On removing the spoon, which I did frequently as an experiment, it was found that inspiratory movements caused the tongue and the epiglottis to valve against the pharyngeal wall. As long as the spoon was in the throat and held in the proper way there was no obstruction to the breathing. Hour after hour a servant held the spoon in the proper position, and it was delightful to hear the air rushing in and out of the chest through the glottis during the movements of artificial respiration. On passing a finger into the mouth behind the spoon it was found that a large cavity existed between the spoon and the pharyngeal wall, and that the air passages through the mouth and through the nares were perfectly free.

Owing to the patient's legs having been dreadfully burnt by hot-water bottles which had been placed on them before I arrived at the house, it was impossible to get her out of the bed. So for twelve hours artificial respiration was kept up. At the end of that time it was considered safe to remove the spoon from the throat and to trust to natural breathing. The lady made a good recovery, and never made a single complaint as to any strange feelings in her throat, proving that the method—by means of the handle of a tablespoon—of raising the tongue and epiglottis for many hours at a time is quite harmless.

I regret very much I did not publish that case and my paper in full at the time. There were circumstances, however, which made it impossible for me to do so. When the Secretary of this Society asked me to send him my manuscript for publication, instead of doing so I sat down and wrote out carefully an abstract of my

paper for publication in the *Edinburgh Medical Journal*, which I knew would embody the important parts of it. Kindly allow me to read that abstract as published, which is as follows:—

"Instead of seizing the tongue with a pair of catch-forceps and dragging it forward, as is usually done when a patient's breathing is arrested during the administration of chloroform, Dr Foulis proposes that one of the curved ends of the glossotilt should be passed down over the dorsum of the patient's tongue as far as the base, and then the instrument should be pressed back in such a way that it acts as a lever. The base of the tongue and the hyoid bone are thus made to project forward. When this takes place, the epiglottis, being closely connected with the hyoid bone, is drawn well away from the rima glottidis, and the aryteno-epiglottidean folds are rendered tense. The instrument is passed into the throat with the greatest ease if the patient's head is allowed to fall well back over the end of the operating table, or the head may be made to fall back by passing an arm under the patient's neck and raising it.

"Dr Foulis maintains that the only effectual way of raising the base of the tongue, of raising the epiglottis, and of making tense the aryteno-epiglottidean folds at one and the same time, is to act on the base of the tongue and the hyoid bone in the manner he has described.

"All are agreed that in any case of impending death during the administration of an anæsthetic artificial respiration is the best means of treatment; but artificial respiration is of little value if air is not made to pass freely in and out of the lungs. The object of the paper was to show that the tongue, the epiglottis, and the aryteno-epiglottidean folds occasionally act as valvular impediments at the orifice of the windpipe to the passage of air in and out of the lungs, and that by the use of a simple instrument, such as the glossotilt, or even the handle of a large spoon, it was possible to raise up the dorsum of the tongue from its contact with the posterior pharyngeal wall, and at the same time to raise the epiglottis, and to render tense the aryteno-epiglottidean folds; and that when this was accomplished there could not possibly be any obstruction to the passage of air in and out of the lungs by those structures during the movements of artificial respiration. It was generally taught by surgeons that dragging on the tip of the tongue with catch-forceps was sufficient to effect all this; but Dr Foulis showed that this was not the case, and that it was not possible to stretch the aryteno-epiglottic folds except by hooking forward the hyoid bone and the base of the tongue; and this could be most easily done by using the simple instrument in the way he had described."

I have read these extracts to-night, just as they were published in the *Edinburgh Medical Journal* nine years ago, in order that you may see that, as far back as 1880, I had devised a simple

means of raising the tongue and the epiglottis at one and the same time, and had already tried it, and found it to be thoroughly reliable and trustworthy, in the case of opium poisoning which I have narrated. Dr Howard, therefore, cannot claim to have discovered a new and only way of raising the epiglottis. Up to 1880, I had read all that Dr Howard had published on the subject of artificial respiration, and I saw then that his method of artificial respiration is not a reliable one, because in it there is no certainty that the tongue may not act as a valve by falling against the pharyngeal wall during the inrush of air. Allowing the head to fall well back over the end of a table until the skin of the neck is made tense will undoubtedly raise the epiglottis, but there is no certainty that the tongue will be raised up at the same time, and unless this is done no method of artificial respiration can be considered a reliable one. I say nothing about Dr Howard's method of chest compression in the meantime. Our attention is at present fixed upon the question, what is the best method of raising the tongue and the epiglottis at one and the same time; and I have attempted to show that Howard's method will not answer, for the reasons stated. There is but one way of raising the tongue and the epiglottis at one and the same time, and this one way is perfect, inasmuch as it makes it absolutely certain that neither the tongue nor the epiglottis can fall back and valve against the pharyngeal wall during the movements of artificial respiration, or during the natural breathing of a deeply anæsthetized person.

If you now look at Plate III., Fig. 3, and place it in such a position that it represents a person lying on his back with the head well back, you will see that the large flabby tongue hangs down, suspended, as it were, from the upper genial tubercles, and that a very little relaxation of its muscular supports will allow it to fall against the pharyngeal wall. The hyoid bone at the base of the tongue is also suspended by muscles from the lower genial tubercles of the jaw, and, like the tongue, if its suspensory muscles become relaxed, it also will fall back against the pharyngeal wall, and as the hyoid bone moves so will the epiglottis and base of the tongue move.

It is scarcely necessary to add that when the lower jaw itself falls backward, from relaxation of its muscular attachments, the tongue and the epiglottis come more easily in contact with the pharyngeal wall.

As air can pass into the lungs only in the channel which lies behind the tongue and the epiglottis, it must be clear to every one that to keep this channel open we must prevent the tongue, the hyoid bone, and the epiglottis from falling back on the pharyngeal wall, and this can be most easily done by lifting forward the whole tongue and the hyoid bone in exactly the same way as I did in the case of opium poisoning, in which it was necessary to carry out artificial respiration for fully twelve hours.

Suppose, now, we have to do with a case in which, from some

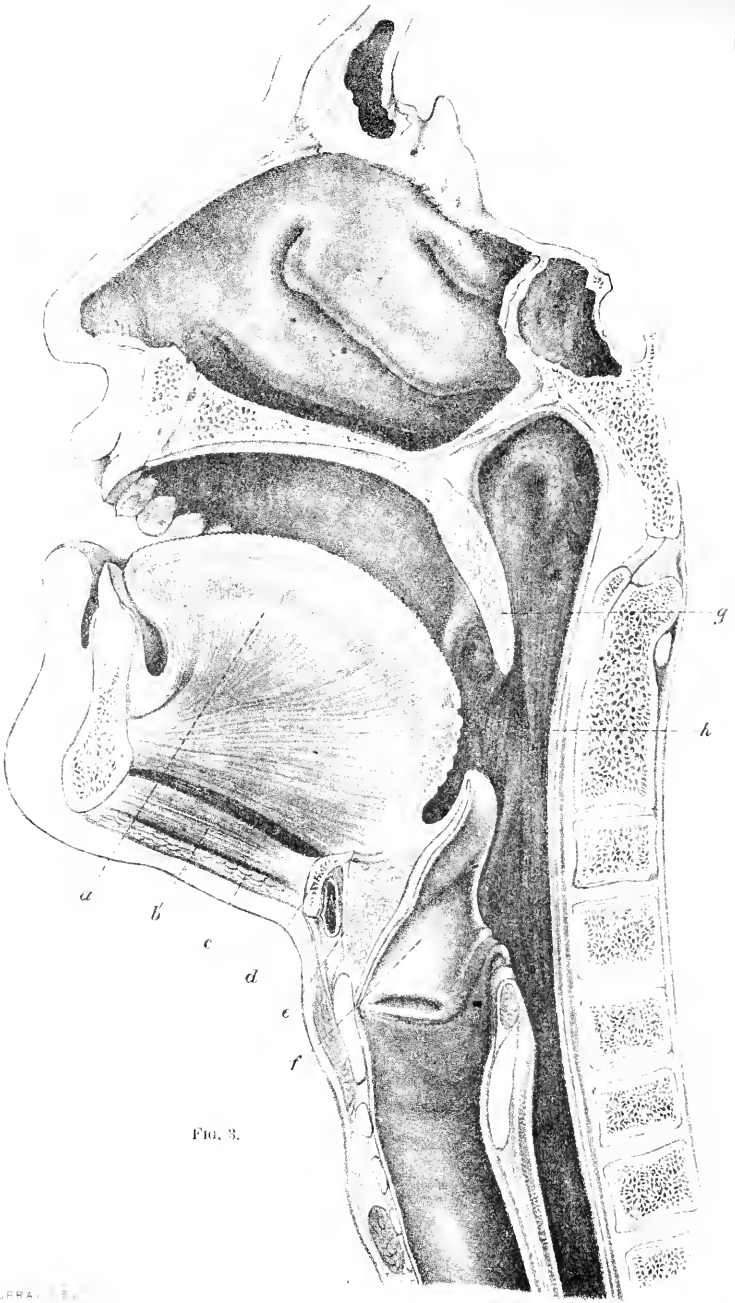


FIG. 3.

Vertical mesial section of head and neck, to show the relation of the tongue and epiglottis to the pharyngeal wall.

a. The tongue and geniohyoglossus muscle.
b. The geniohyoid muscle.
c. The mylohyoid muscle.
d. The hyoid bone.
e. The thyro-hyoid membrane.
f. The epiglottis and right arytenoepiglottic fold.
g. The soft palate.
h. The post-pharyngeal wall.

(From Luschka.)

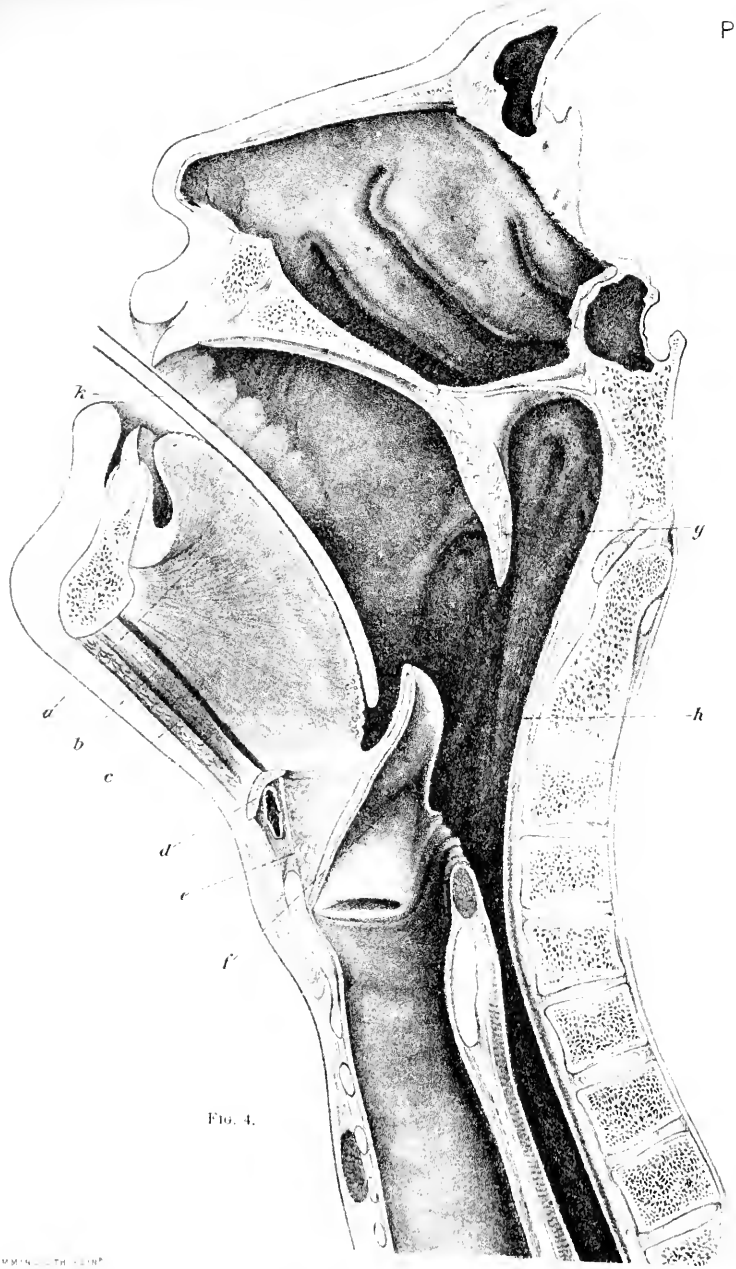


Fig. 4.

Vertical mesial section of head and neck, to show the free breathing-space through the mouth and nose, when the whole tongue is lifted forward by means of the handle of an ordinary table-spoon. On depressing the spoon, the base of the tongue and hyoid bone move forward.

a. The tongue and geniohyoglossus muscle.
b. The geniohyoid muscle.
c. The mylohyoid muscle.
d. The hyoid bone.
e. The thyro-hyoid membrane.
f. The epiglottis and right arytenoepiglottis fold.
g. The soft palate.
h. The post-pharyngeal wall.
k. The handle of a large spoon, lifting forward the whole tongue with the epiglottis.

(Partly from Luschka and from Nature.)

cause or other, it is necessary to carry out artificial respiration to bring back the insensible person to life, the first thing to be done is to cause the patient's head to fall well back over the end of the table or bed, on which he may be lying; or two pillows may be placed below his shoulders, and the head allowed to fall back naturally. There are other ways of making the head fall back, but I need not speak of them now.

In the next place, it is the simplest thing in the world to insert an instrument into the air-passage which lies between the tongue and the pharyngeal wall (Plate IV., Fig. 4). A forefinger may be passed in, or any flat instrument, such as the handle of an ordinary tablespoon or a fork, *k*. It is only necessary to insert such an instrument between the teeth, and then push it onwards on the surface of the tongue till it comes in contact with the pharyngeal wall. The end of such an instrument will be now found to be near the base of the tongue. As the head of the patient is bent well back the lower jaw is carried out of the way, and if the handle of the spoon is now pressed back, the whole tongue will be carried forward, and all parts which lie in front of the handle of the spoon may now be lifted forward with great ease; and it will be found that as the instrument is levered backwards its point glides forward on the surface of the tongue until it strikes on the middle glosso-epiglottic ligament, and then fits into a sulcus at the point of junction of the tongue with the hyoid bone. The hyoid bone can now be made to move forwards as far as the thyrohyoid membrane will allow it, and the epiglottis will, of course, move with it. A very slight amount of force is sufficient to do all this, and it can be done in a second or two of time. Now, what is the state of matters after this is done? It is this,—the whole tongue down to its base has now been lifted forward as far as the thyrohyoid membrane will stretch, and as a result the tongue is well removed from all danger of contact with the pharyngeal wall; the epiglottis is also raised, and the aryteno-epiglottic folds are put on the stretch, the mouth is quite open, and air can now pass into the lungs through the mouth as well as by the post-oral channel, and during the movements of artificial respiration none of these parts can possibly act as valvular impediments during the inrush of air. There is no theory in all this. The method has been thoroughly put to the test, and it has never failed. It is applicable in every case where it is necessary to remove those causes of obstruction to breathing such as I have described, and it is applicable in every case where it is necessary to carry out artificial respiration after these causes of obstruction have been removed.

But some one may say we agree with all you have said as regards the mechanical obstruction caused by the tongue and the epiglottis, and as regards the means proposed to relieve this obstruction, but you have not sufficiently considered that form of vital obstruction which seems to occur within the box of the larynx, and

which is accompanied by a peculiar inspiratory laryngeal stridor, and is sometimes met with in the early stages of chloroform administration. It is difficult to see how the true or false vocal chords can cause serious obstruction to the breathing during the administration of chloroform; but, undoubtedly, in these cases the noise we hear seems to be vocal. It is a very alarming noise, and the patient may become somewhat cyanosed in such cases. We have often seen that dragging on the tip of the tongue gives instant relief in these cases. How is the relief thus brought about? I suspect it was in these cases that Lister taught that the relief to the breathing was brought about in a reflex manner, though it is not easy to follow the physiology of the process.

There are cases, however, in which dragging on the tip of the tongue with catch forceps does not put a stop to the laryngeal stridor, though it gives relief to the laboured breathing, judging from the accompanying improvement in the patient's face.

In regard to these cases I have always held the view that the obstruction to the breathing is more or less mechanical,—that is to say, the obstruction which makes the patient look cyanosed is not caused by the laryngeal vocal chords, but by the tongue falling back at the same time that the laryngeal stridor is going on. Hence dragging on the tip of the tongue removes the mechanical obstruction, but does not always remove the laryngeal stridor, which, however, will pass off if we can continue chloroform administration until reflex action is abolished. I have found that lifting forward the whole tongue and the epiglottis, and making tense the aryteno-epiglottic folds by means of the glossotilt, removes the obstruction to the breathing in these cases just as effectually as does dragging on the tip of the tongue with catch forceps.

As I have already said, all this can be done with the forefinger, but there are many objections to the forefinger being used, and as long as we can get hold of a tablespoon or a fork, or, in fact, any instrument to pass down into the throat as far as the base of the tongue, we need not use the forefinger. Now, my object in reading this paper is not to recommend any special instrument, but rather the simple method by which, under all conditions of apnœa, we can effectually raise the tongue, raise the epiglottis, and make tense the aryteno-epiglottic folds at one and the same time, all of which parts are liable to interfere with the breathing in an anæsthetized or unconscious patient, and all of which may greatly interfere with a successful result in artificial respiration.

At the same time, it is advisable to point out that there is a form of instrument that experiment on the dead and living body has shown to be extremely easy of application, and therefore I bring it before you this evening (Plate V., Figs. 5 and 6, *k*). The first instrument I used for tilting forward the base of the tongue was very like a double-handled tablespoon with the ends slightly curved; this answered very well; but from numerous experiments

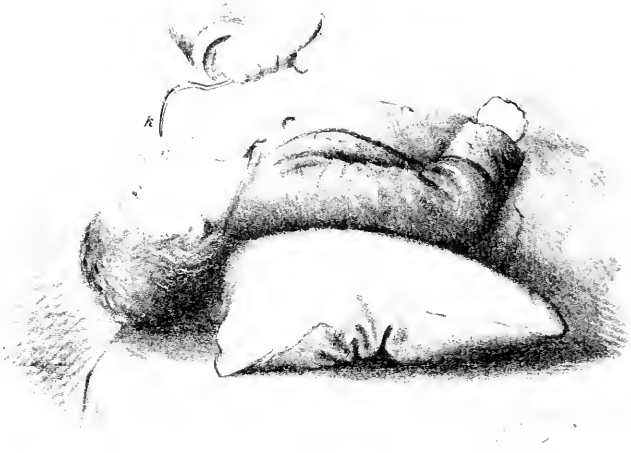


Fig. 5.

Fig. 6.

Showing the manner of using the glossotilt *k*. The point of the instrument is inserted between the teeth, and as it is pushed onwards, by a simple turn of the wrist (see fig. 6), it is made to lift forward the whole body of the tongue, and, at the same time, the hyoid bone and the epiglottis are carried forward.

J. T. MURRAY DEL.

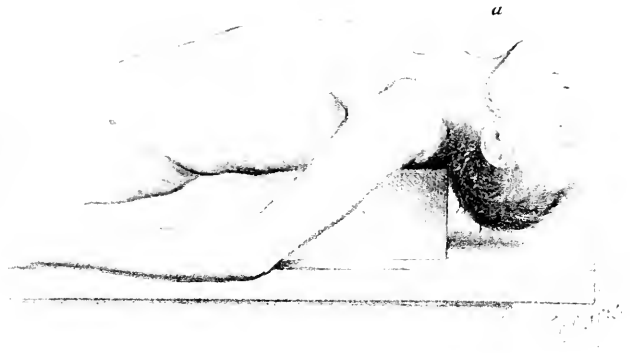


Fig. 7.

From a photograph of a dead body, showing the curved line (*a*) that exists between the chin and the upper border of the thyroid cartilage in the middle line. In this case examination showed that the tongue was lying in contact with the pharyngeal wall.

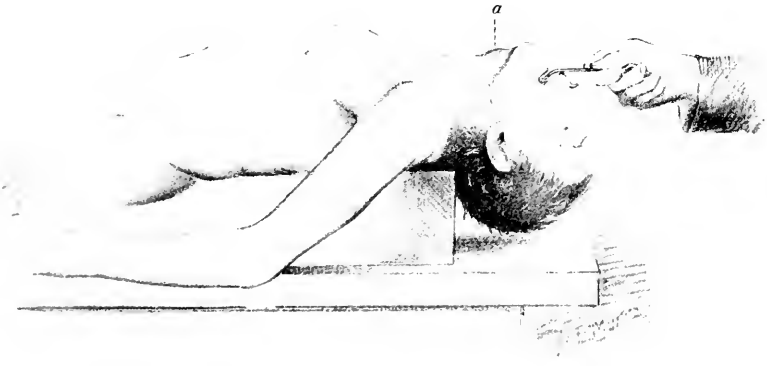


Fig. 8.

A photograph of the same body, showing the appearance of the neck below the chin in the middle line (*a*) when the glossotilt is held in proper position during artificial respiration. In this case examination showed that the whole tongue was well removed from the pharyngeal wall, and that the epiglottis was raised, and the arytenoepiglottic folds were quite tense.

McLAGAN & CUMMING, LITH. EDIN.

made lately, I have found that this instrument, which is called a glossotilt, answers best of all. It is simply a double-handled tablespoon, but the ends are so curved that when the point is inserted between the teeth, the curved end adapts itself to the roof of the patient's mouth, and a very slight amount of levering backwards brings its point into the sulcus which lies at the base of the tongue above the hyoid bone, and the end being well curved the whole body of the tongue lies in the hollow of the curve. The tongue as a mass can in this way be lifted forward with the greatest ease. In inserting it into the patient's mouth we simply keep the point in contact with the roof of the mouth, and push it onwards. It will, in consequence of the curve, find its own way to the base of the tongue, and then the whole tongue can be lifted forward until the thyrohyoid membrane is put on the stretch.

If you look at Plate VI., Fig. 7, *a*, which is a photograph in profile of a dead body, you will see that the line of skin which lies between the symphysis of the lower jaw and the prominent edge of the thyroid cartilage is a curved one. In this case, examination has proved to us that the tongue and the epiglottis have fallen back, and are in contact with the posterior pharyngeal wall. Now, look at Fig. 8, *a*, which is a photograph of the same body, but, in this case, the glossotilt is in position, and you will notice that the part of the neck which lies between the symphysis of the jaw and the thyroid cartilage is now bulged forward, and a finger passed into the mouth behind the glossotilt shows us that the tongue and the epiglottis are well lifted forward away from the pharyngeal wall, and that the breathing channel through the mouth and through the nose is quite free. Air may now be pumped in and out of the chest in this body by Howard's method of compressing the chest; and it is quite striking to notice how freely air passes in and out through the glottis, according as the chest is compressed or allowed to expand, and it will be clear to everybody that it is quite impossible for the tongue and epiglottis now to act as valves as long as the glossotilt is in its proper position. I might tell you of many cases in which I have used the glossotilt during the last 8 or 9 years, but this is not necessary. In the case of young children, and in newly born infants, the handle of a dessertspoon or a teaspoon may be used as a means of raising up the tongue and epiglottis, while artificial respiration by Howard's method is being carried out. In one case of a newly born infant I used the handle of a teaspoon to lift up the tongue and epiglottis, and air was made to pass freely in and out of the chest with one hand, while the other hand held the teaspoon in proper position.

Much might be said in favour of this method in the case of the apparently drowned, when artificial respiration has so often to be carried out energetically. I fear many lives have been lost in all parts of the world through not raising up the tongue and epiglottis properly in such cases of artificial respiration, and yet how simply

this can be done when you know how to do it. A coat or two folded up and placed under the sufferer's shoulders would allow the head to fall back. A tablespoon or a fork is generally at hand, and if it is not, any straight instrument could be used in the same way as the handle of a tablespoon. Of course the fingers can be used to lift up the whole body of the tongue in any case when the patient is deeply insensible. In using the handle of a tablespoon or of a fork, if the head is bent back, such an instrument can be passed to the base of the tongue without the slightest difficulty. The sword swallower has no difficulty in passing his sword down his own gullet. When the head is well bent back the passage from the mouth to the gullet is almost a straight tube, and it is very easy to pass a straight instrument down this tube. And after the lower jaw has, in this way, been well raised and fixed, the head itself with the upper jaw can still be carried back a considerable distance, moving, of course, on the articulation that exists between the condyles of the occipital bone and the atlas vertebra. It is well, also, to point out that in using the glossotilt it is not necessary to bend back the patient's head over the end of a table or over a pillow. If the patient is lying on his side, as in chloroform administration, on a table, or on the bed, the head can be bent back sufficiently to pass the glossotilt down behind the tongue as far as its base, when a very slight amount of backward pressure with the instrument will now cause the tongue to bulge forward in the neck below the lower jaw; and when this is done we may be sure that the tongue is carried forward as far as the thyrohyoid membrane will allow it, and that it is well removed from the pharyngeal wall. This act of carrying forward the whole tongue as far as the thyrohyoid membrane will allow it, also raises up the epiglottis, and makes tense the aryteno-epiglottic folds.

Howard's method of compressing the chest in artificial respiration, combined with this simple way of raising the tongue and the epiglottis, should be taught in all our public schools and in our ambulance classes. One may be called upon at any moment to assist in saving the life of a fellow-creature who has fallen into water, or who is in danger of death from want of oxygen, and if we know how to carry out artificial respiration in a proper manner without delay, many a life may be saved by such knowledge. I might have discussed the respective merits of Howard's, Marshall Hall's, and Silvester's methods of artificial respiration, but from what I have already said you will see that I prefer Howard's method of compressing the chest, combined with the proper method of raising the tongue and the epiglottis, to all others. My own experiments on the human body have convinced me that it is the most easy to carry out, while it is the most effectual. In no case have I ever heard a single complaint of injury being done to the soft parts by the use of the glossotilt. As I have already said, my object is not so much to recommend this instrument as to insist

that there is but one safe and reliable way of raising the tongue, of raising the epiglottis, and of making tense the aryteno-epiglottic folds at one and the same time, and you may use any instrument you please to do this.

In one case I used the handle of an ordinary paper cutter, and in another case I could lay my hands on nothing but a gentleman's boot hook. This was passed with the flat side against the tongue as far as the hyoid bone, which was then tilted forward until the thyrohyoid membrane was put on the stretch. The patient was choking at the time, and instant relief was given by thus lifting forward the hyoid bone and the base of the tongue.

The important thing in such cases is not the instrument, but the knowledge of what ought to be done, and how to do it.

Mr John Duncan said the subject of this really admirable paper was one on which those who had experience in the giving of chloroform might dilate for hours. When Mr Howard did his spread-eagle business in London the other day, they should all have been prepared to accept his paper as a well-reasoned exposition of the anatomical facts connected with the subject of respiration under chloroform; but some of them would take very strong exception to his "new and only," and he (Mr Duncan) rather regretted Dr Foulis's use of the word only. Mr Howard's method was not new, nor was it the only one, nor was it very good. The practical application of his well-reasoned observations was not satisfactory. He supposed that under chloroform the risks came solely from the respiration, and they came in various ways. There was a way in which the respiratory act simply died out without any obstruction to the respiration. With that method of death (it was very important to distinguish the various methods) they had not then to deal. Another method with which also they had little to do was the risk of solids and fluids getting into the air-passages, even thin fluids lying in the back of the pharynx having a great influence. The modes of death with which they were concerned that evening were those in which obstruction took place from a paralysis causing an obliteration of the upper air-passages. He supposed this occurred in two ways. One was paralysis of the intrinsic folds at the opening of the larynx, in which a sort of valvular action took place, and the other, to which Dr Foulis drew their attention, was the falling back of the tongue and blocking up of the pharynx. Now these two conditions might be easily distinguished in the experience of any one who had seen risk arising from them, and who had adopted the method of pulling up the tongue. In the one case the arrestment of respiration was relieved by the tongue being pulled slightly forwards. But in the other class of cases, in which there was more complete paralysis of the tongue and of the orifice of the larynx, they required to pull very forcibly on the tongue to relieve it. They now came to the question of how best to over-

come those difficulties. He was prepared to maintain that they were both capable of being overcome by pulling on the tongue from the tip; and they were also capable of being overcome, though not so completely, by pushing forwards the angles of the jaw; and he was prepared to admit that Howard's method, if carried out as he described it, was also generally effectual in doing the same. How the pulling on the tip of the tongue acted he was not prepared to say, but he had suggested to Dr Foulis that it might be by putting the fibres of the genio-hyo-glossus and other muscles on the stretch, and so, both mechanically through traction on the jaw and reflexly by causing them to contract, raising the hyoid bone. From all he had seen and heard, he was strongly inclined to adhere to the old method, which was extremely effective, and required no instrument beyond what was in their pocket-cases, and really produced very little injury to the tongue. He had, at the same time, no doubt Dr Foulis's instrument was perfectly effective. As to the modes of artificial respiration, he immensely preferred Sylvester's method to Howard's. He believed the air entered in greater volume and was changed more rapidly in the former than in the latter method.

Dr M'Bride failed to see how in any narcosis the ary-epiglottic folds could be paralyzed, and how even if they were they could affect the respiration. The chief obstacle lay in the base of the tongue, and the epiglottis falling against the posterior pharyngeal wall. Theoretically it seemed to him that any of the methods they had discussed would be efficacious in getting these apart.

Mr Cathcart pointed out that in addition to the deep guttural sound emitted by patients under chloroform there was another of a crowing character, which seemed to support Sir Joseph Lister's view that the obstruction was sometimes in the larynx. He ventured to disagree with Mr Duncan's opinion that pulling on the tip of the tongue was in every case successful. He had seen cases in which the patient was in imminent danger, in which efforts to pull forward the tongue had failed to give relief. He observed this in one instance in Mr Joseph Bell's hospital practice, in which after tongue-pulling failed the finger passed down to the epiglottis and hooking forward the tongue was of immediate benefit. If the advantage of pulling forward the tongue were that of exciting reflex action, one could imagine that in deep narcosis the reflexes might be abolished, and in such a case the advantage of Dr Foulis's mechanical method would be apparent. With regard to the methods of artificial respiration, he was inclined to think Sylvester's superior to Howard's. In the latter expiration alone appeared to be produced, whereas in Sylvester's the raising of the arms above the head produced inspiration and the lowering of them to compress the chest-walls expiration.

Dr James Ritchie had seen cases like those referred to by Mr Cathcart, in which pulling the tongue forward failed to give relief,

but pulling up the jaw did. In contrasting the methods of artificial respiration, he thought Mr Cathcart had overlooked the fact, that in the Howard method the arms were fixed above the head in such a way as to place the chest in the condition of full inspiration. One of the great advantages of the Howard method was that, if it had to be undertaken single-handed, it could be kept up without fatigue for a very much longer period than Sylvester's, and therefore it was a suitable method to teach the ambulance and swimming classes.

Mr A. G. Miller said the practice of pulling forward the tongue had originated with a house-surgeon of his father. A slight operation was being performed, when the respiration suddenly became embarrassed. The house-surgeon surmising that the tongue had something to do with it, seized the tip with a pair of Liston forceps, and the breathing was at once relieved.

Dr Symington showed, in connexion with Dr Foulis's paper, two horizontal sections through the head of a male subject, made while the body was frozen and lying on its back. These showed the parts referred to in the paper. They showed that after death the tongue was very apt to fall back just as in chloro-narcosis. A considerable part of the dorsum of the tongue was lying in contact with the posterior pharyngeal wall, and the pharynx itself for about an inch above the epiglottis was little more than a transverse slit. It seemed to him that even a comparatively slight pull on the tip of the tongue would move it forward sufficiently to turn this transverse slit into a considerable cavity. So far as Dr Foulis's method went, it seemed to him an admirable one, not only by drawing forward the tongue, but by keeping it there. He did not think Dr Howard's experiments with water in the fossæ between the tongue and the epiglottis established anything. He agreed with Mr Cathcart as to the obstruction being sometimes in the larynx, and the experiments of Dr Wyllie helped them to understand that. In the majority of cases he believed the obstruction was due to the tongue falling back.

Dr P. A. Young said he was interested in Dr Foulis's paper from an ambulance point of view. Ambulance students were generally taught the Sylvester method of artificial respiration, although they were also sometimes instructed how to employ Howard's method. He thought that while the glossotilt might be an admirable instrument in skilled hands, it would prove dangerous if used by ambulance pupils. He did not think that Dr Foulis had correctly represented the Howard method of artificial respiration. The arms ought to be raised and fixed above the head.

Dr Foulis, in reply, said,—With regard to any supposed injury to soft parts in using the glossotilt or handle of a tablespoon, he had not met with an instance of such during the last nine years, and did not see how such could occur, as only the slightest amount of force was necessary to raise up both the tongue and epiglottis.

Far less injury was done to the soft parts with the glossotilt than with the catch forceps, for in using this last instrument the tongue must be dragged upon violently if the method is to be effectual, and, according to some gentlemen who had spoken to-night, it was not always effectual in relieving the breathing even then, but it injured the tongue. As regards artificial respiration, Dr Foulis would again say that Howard's method was the best of all; and as to the question of the quantity of air pumped in and out of the chest in that method, he would like to point out to Dr Cathcart that as much air rushed in as was pumped out by the act of compressing the chest. All other methods were very exhausting to the operator, while Howard's method was not so, and could be carried out with ease for hours at a time. It was surprising how large a volume of air could be made to rush in and out of the chest by Howard's method when the tongue and the epiglottis were kept out of the way by means of the glossotilt; and he would ask gentlemen present to give this method a trial before condemning it. He would also point out that in artificial respiration air should be pumped in and out of the chest as rapidly as possible at first, but that as soon as signs of natural breathing were established, the artificial movements should accompany the natural movements of the chest. In conclusion, Dr Foulis said that from all he had heard from the speakers that evening, he thought he was quite right in describing his method as the only way of raising the tongue, of raising the epiglottis, and of stretching the arytenoepiglottic folds at one and the same time.

Meeting VIII.—March 20, 1889.

Dr John Smith, *President*, *in the Chair.*

I. Exhibition of Patient.

Dr Caverhill showed a case of Addison's disease in a young German commercial traveller. Within the last eighteen months he had developed a bronzing of the skin, which was well marked on the face, gums, epigastrium, and genital organs. There was a history of a severe wrenching of the body while skating some sixteen months ago. He had no special feeling of weakness, led an active life, and his weight, which was rather small, was increasing.

II. Discussion on the Pathology and Treatment of Strumous Glands.

The President.—The business to-night is a discussion on the pathology and treatment of strumous glands, and I daresay it

will be admitted that this is an affection of sufficient interest, frequency, and importance to warrant a special meeting of the Society such as has been called for this evening. I hope that as many gentlemen as possible will give their opinions or their experience in the matter. Many different views have been entertained of the pathology of these lesions,—an inherited instability of tissue in certain organs prone to be attacked, and among these especially the glands and lymphoid structures; the inability of such weakened structures to resist the effects of inflammatory processes, coupled with low recuperative power; the influence of what is called scrofula, with or without the agency of tuberculous elements; the action of bacilli,—all these and their effects upon the gland tissues present themselves for consideration on the pathological side of the question, while the treatment of a malady leaving not only disastrous constitutional changes as its after-effects in some cases, but the extremely unsightly cicatrices so often visible, equally render it an interesting subject for attention. I call upon Professor Annandale to open the discussion.

Professor Annandale.—Mr President and Gentlemen, you have done me the honour to ask me to be one of the leaders in this important discussion. I feel placed in a rather responsible position. It is, I think, the duty of one placed in my position to-night to endeavour, as far as possible, to indicate and limit the lines of discussion; but in connexion with this subject I feel it may be somewhat difficult to do so. There are so many side issues which are tempting to pass into; but I shall do my best to keep to the main lines. Necessarily I shall have to refer to the minute or microscopic anatomy and pathology of this subject. I do not wish to say very much about this, for there are gentlemen here who have a much more complete knowledge than I have. There are two favours I should ask. The first favour is, that if in my remarks I say anything inconsistent with recent pathology, they will at once correct me. The second favour I have to ask is, that in their remarks they will endeavour as much as possible to summarize or arrange their facts, so as to be useful in connexion with the diagnosis and treatment of this disease. At the very commencement of this discussion a difficulty arises which will occur to most of you, the definition of the term "strumous glands." I believe, and hope I am correct in stating, that the word "struma" is now considered synonymous with tubercle or tubercular disease, but unfortunately this term is still used in a wider and more indefinite sense, and this constitutes one difficulty. Another difficulty in my mind is, that even if we take what is termed tubercular by pathologists, it does not include a certain class of case, known to observers, which resembles the tubercular affections in its behaviour and yet is not tubercular. I will refer, in the first place, to what is considered now as tubercular

affection of the glands. Clinically, I take it that a tubercular condition of the glands is considered to be that in which glands in certain situations become enlarged. The glands in the neck, the glands in the thorax and abdomen, are from experience known to be much more frequently affected than glands in other situations. One explanation has been given, that in connexion with the naso-pharynx, lungs, and intestines, there is a greater amount of adenoid tissue in these parts, and it is in connexion with irritations of these tissues that the glands are more often enlarged. It is quite possible there is another explanation; that we may find that some of these micro-organisms are more commonly connected with the naso-pharynx, lungs, and intestines, and may have an influence in producing this tubercular condition.

In the next place, we have the fact that one or more glands may be affected—sometimes one, sometimes several. Sometimes one gland appears to be a source of infection to others, the infection being carried along the small arteries, the lymphatics producing a more or less general tuberculosis. Then, again, it is a known fact, acknowledged even by pathologists, that the tubercular condition is not unfrequently simply local; a well-marked tubercular condition, examined microscopically and also observed clinically, may remain local; whereas, in other cases, the condition gradually develops into a general tuberculosis from infection. In connexion with the microscopic pathology of tubercle, I understand that the diagnostic and distinctive symptoms, pathologically, of tuberculosis are,—Firstly, the presence of a certain grouping of cells—giant cells; secondly, we may have the presence of the micro-organism or bacillus; lastly, the condition of caseation. These are the facts pathologically in regard to tuberculosis. When we come to consider the affections of the glands clinically—and I do not refer to special affections such as cancer or syphilis—we certainly meet with conditions which very much resemble those conditions usually termed tubercular, but are certainly not tubercular; and therefore it occurs to me that any term, such as scrofula, struma, or tubercular affection of the glands, is a matter of confusion. I cannot help thinking it would be better simply to look upon all these affections of the glands as inflammatory, and I believe most authorities are agreed that all these processes—even tubercular—are of the nature of an inflammation. Well, then, I would suggest that instead of using the word "tubercular" gland or "scrofulous" gland, we use the general word adenitis, and by careful observation and pathological research we endeavour to specify or classify the different forms of adenitis. It will probably come to be that we shall have a simple suppurative adenitis, tubercular adenitis, scarlatinal adenitis, etc. Well, then, I should prefer, in the present state of our knowledge—certainly by no means perfect—that we should class these affections of the glands as simply "inflammatory," and by careful research, both clini-

cally and pathologically, endeavour to classify the different forms of adenitis.

Next, the important question of causation. I prefer to consider the question not as simply tubercular, but that we meet with a certain group of inflammatory affections of the glands modified by certain circumstances. And these circumstances are—(1), The conditions of the patient (and we do not know yet what all those conditions are); (2), local irritation of various kinds; and (3), the special tissue or organ, and, in connexion with the glands, the special glandular texture in which the disease originates must be taken into consideration. If we do this we are in a better position to gain light by further observation. In regard to the causation, the causation of these conditions may be divided, in the first place, into local causes. These local causes are very much more numerous than is generally supposed. All forms of irritation will produce the inflammatory conditions of glands as we know, but still there are special forms of irritation that undoubtedly produce some of those causes in the group to which I have referred. We have always to remember that in addition to local causes, we have the general or constitutional cause —the condition of the patient. There are many of these local causes which in a healthy patient would have no effect, but in certain conditions of health with which we are not thoroughly acquainted, the various local irritations which, perhaps, would cause very little effect in a healthy patient, will cause a glandular enlargement when that special constitutional condition is present. Then in regard to the organ or tissue in which the affection arises or exists, we must always take into consideration, I think, in studying the disease, the organ or tissue in which this disease exists; because every organ or tissue has its function in the human body, and we can quite understand in the case of a gland, which has certain functions to perform, and is liable to certain special forms of irritation, the glands may be affected by some general condition of health affecting the function of that gland.

Now I pass to a matter in which I feel more at home, and that is the treatment; but in connexion with the treatment one has to remember the causation, and the first point in connexion with the treatment of these affections—and, mind, I do not limit my remarks to simple tubercular affections, I rather prefer to look upon the subject as consisting of a group of cases not always tubercular. The first remark I shall make is, that every case ought to be carefully studied by itself, because I believe there has been far too much routine in practice. We all know what the usual practice is in the waiting-rooms of the Infirmary or elsewhere. The patient presents himself with some enlargement of the glands of the neck, and, without much study, it is declared to be a case of strumous affection of the glands, and cod-liver oil internally with iodine externally ordered. But every case ought to be considered by

itself. Every case has usually a special local cause and a general cause. The first thing to be done in treating a case of affection of the glands (I prefer to call it inflammatory affection) is to carefully study the case, and more particularly to endeavour to find out whether or not there is any local irritation which can be removed. I have, over and over again, seen in private practice delicate young children, or even adults, suffering from enlargement of the glands of the neck; and these enlargements would at first sight be very naturally pronounced strumous or scrofulous or tubercular, but on further examination I have found that these were cases which depended upon a local cause. Quite true, the condition of the patient may have made the affection more liable to occur; but the proof that it was the local cause was that whenever that was removed the glandular affection got better. I will mention one cause, and that is irritation connected with the teeth—not necessarily decayed or bad teeth, but "jammed" teeth, teeth too crowded together. I have seen one or two cases in which the glands have remained enlarged for months and months, and have been treated as strumous or scrofulous, and yet the glands have gradually got well after teeth have been extracted and tension relieved.

I think it also important in connexion with the disease of the cervical glands that the whole naso-pharynx and the ear should be carefully examined, because these local conditions are probably the cause of the irritation of the glands. Then, having got rid of this question of local irritation, we come to the question of general treatment. This must not in all cases be cod-liver oil or even maltine; but the patient's general health should be investigated, and treated according to its condition. When the surgeon is called upon to see a case of enlarged or diseased glands, one very important thing is to find out whether or not there is suppuration, and if there is suppuration, I have no hesitation in saying that that should be at once relieved. But even before the stage of suppuration is reached, I have also no hesitation in saying that, if a case of enlargement or disease of the glands presents itself, local treatment, more especially soothing treatment, should in the first place be tried, provided there are no signs of suppuration; then counter irritation should be used. If that does not relieve—more especially if there should be tension—puncture, done with a tenotomy knife, is often very valuable. In our present state of knowledge, I have not much faith in injections, simply for this reason, that, as far as pathological and clinical knowledge go, I do not think we can diagnose whether a gland is in a tubercular state or in one of mere simple inflammation by simple external examination. If a gland is well marked with tubercular disease, I think no injection of iodine, or no injection of anything else, will be useful. So that if soothing treatment, if the removal of the cause, if the treatment of the constitution and general health, if the application of soothing means locally, or counter irritation fail, I think puncture should

be tried by introducing a Graefe knife at two or three points. At the same time, in all these cases I should be careful in watching for suppuration; and as soon as suppuration is detected an incision should be made—in the first instance limited—and this allows you to examine the gland; and in the majority of cases you will find the gland is diseased. In other cases you will find the gland itself suppurating; and if this is present, the sooner you get rid of the gland the better, and it should be got rid of entirely. You can sometimes scrape the diseased gland away, and I would say the scraping must be thorough. The ordinary Volkmann spoon is not always the best instrument, because in many of these cases you will find that the gland is situated in some depth; so that you will require an instrument more or less curved, not simply a straight one, but a curve which will allow you to get thoroughly in and scrape away at the entire gland. There are other cases in which excision of the gland is advisable, and I think that it is so in cases where the gland is not very adherent or has resisted all treatment, or in cases in which the gland has remained for a long time without undergoing any change. In all these cases I believe, provided the gland can be excised safely, that the complete excision of the gland is the best thing that can be done to the patient, so that the treatment which I advise is as follows:—In the first place, remove any local cause; in the second place, treat the general health not simply by cod-liver oil, but by remedies suitable to the particular case; in the third place, apply soothing treatment to the gland itself. If that does not relieve, try puncture, always examining for suppuration; and if that takes place, the entire removal of the gland by scraping. The last point in connexion with the treatment I would refer to is the reparative treatment—that is to say, the removal of cicatrices or the removal of deformity the result of cicatrices. I have had to operate on a very large number of these cases. There are two methods of operating (and I would remark that it is important that the whole of the diseased gland should have disappeared. There is no use attempting to remove cicatrices or deformity until all inflammatory action has subsided). There are two methods of treatment—the first, the method of Mr Adams, who suggested that you should introduce a tenotomy knife and separate the cicatrix from its adhesion to the subjacent textures. But I do not think that this plan is successful when the cicatrix is extensive. I have frequently cut out the cicatrix. It is very fortunate that many of them are in the direction of the lines of the neck. What I advise and practise is to cut out the cicatrix, and then to carefully bring together, by small sutures of horsehair or other substances, the edges of the wound.

John Duncan.—I am in an unfortunate position, because I was unable to hear the earlier parts of Mr Annandale's speech. I am sure the Society will excuse me when I tell them that I was called

to the Infirmary, and have been detained there. The subject is one which cannot, of course, in the time which one may legitimately occupy, be at all exhaustively considered. I shall merely endeavour to refer to one or two points on which Mr Annandale has not dilated to any extent. Taking first the pathological aspect of strumous glands, I suppose that we may nowadays safely enough consider that the disease is really due to the action of the tubercular bacillus, and that the difficulty to which Mr Annandale referred is truly one of diagnosis. Necessarily, therefore, the pathology of strumous glands includes the whole pathology of tubercle. I shall leave that to the pathologist; but there is one point in the etiology which seems to me interesting and important, viz., supposing, as I have supposed, that they are due in every instance to the presence of the tubercular bacillus, how did the bacillus get there? There are plainly only two ways in which the disease can possibly have entered the glands—one is by the lymph path, and the other by the blood path. Mr Annandale, from what I caught of his remarks, seemed rather to favour that which is the more general view, the entrance of the disease by means of the lymph path. This view is more generally entertained because it appears adequately to explain the frequency of tuberculosis in glandular texture, as well as the frequency of tuberculosis in the glands of the neck more especially, to which it may so easily enter by the teeth, the tonsils, and other parts. I am strongly impressed with the idea that this is erroneous, and that the entrance of the disease into the glands is frequently by the blood path. And the reasons which make me think that it is so are briefly these:—First, we have the analogy of tuberculosis of other organs, we have tubercle occurring in many other organs to which it can only arrive by the blood path—such organs as the joints, for instance, the bones, the testicle. I leave again to the pathologist the question as to how it does enter the blood, and how, having got into the blood, it reaches these particular organs, but I picture to myself hypothetically a condition of affairs in which the tubercular bacillus, having arrived at the capillary portion of the circulation, makes a diapedesis like the white corpuscles, and if it then perhaps find a particularly suitable soil, breeds there; or if not, is destroyed by the cells of the tissue in which it finds itself. Another reason then for believing, from the analogy of other organs, in its arrival at the glands by the blood path lies in the curious fact that there is most undoubtedly a selective property in tubercle—it has affinities for particular organs. It would be detaining you too long to adduce many instances of this. Let me merely refer to one or two. For instance, it is plain that the joints afford a particularly suitable soil for the multiplication of the tubercle bacillus, and that in them alone, under many conditions, the tubercle will breed. Thus you have a condition of affairs which I may venture to call strumous or tubercular arthritic fever, a disease which is not described in the books,

but which I have been able to verify, and which very closely resembles rheumatic fever, differing from it in the somewhat shorter duration of the intensely feverish condition, and being characterized by a considerable swelling, comparatively slight pain in the joints, and high fever, subsiding in a week or fortnight, but leaving behind one or two joints to run the particular course pathognomonic of strumous joints. Then another variety of strumous arthritis occurs—a variety, again, not very definitely described in the books—a variety which follows the surgeon's interference, cases in which you perform an operation here or there, generally upon joints, and the bacilli getting into the vessels you have opened descend upon a joint elsewhere. Twice my interference was followed by a very distinct attack in other joints, and in joints only. Arthritis is not alone; many tissues of the body are affected to the exclusion of others. The analogy of these various effects of tubercle upon other organs would lead one, *à priori*, to suppose that it was exceedingly likely that in the glandular textures such might also occur, and this is confirmed by various facts in connexion with strumous or tuberculous adenitis. For example, there is undoubtedly a considerable number of cases in which strumous adenitis is not confined to the glands of the neck, but is found in the axilla also. It is frequently observed that tubercular or strumous glands of the neck have a symmetrical tendency, in chronology appearing about the same time. Strumous glands in the neck are very frequently unaccompanied by any discoverable breach of surface in the parts with which they are connected. Lastly, strumous glands are very rarely (and this is an interesting point) caused by local strumous disease. I have seen three cases of that rare disease, tubercular ulcer of the tongue, two of which were verified by microscopic examination. In these the glands were not affected. Strumous diseases of the joints are not prone to produce disease of the glands. All these facts, I think, point to the probability that strumous glands, even in the neck, are often affected through the blood. I do not mean to deny, on the contrary I am sure that it is true, as Mr Annandale pointed out, that local irritation plays a part—it plays an important part—in the production of strumous glands, but this part, that it forms the soil. A Russian lady doctor has made a most important examination on that particular point, showing that the scarlatina organism has this property, that it prepares, in a very remarkable way, the tonsils and glands of the neck for the action of the ordinary pyogenic organisms in a very virulent form. So it is with pyogenic organisms in connexion with tuberculosis; I am certain they are adjuvant the one to the other; and the surgeon has very frequent opportunities of verifying that fact when he sees the rapid progress of tuberculosis after the occurrence of septicity. In this way it may be found that strumous glands in the neck are actually connected with local sources of irritation, because soil is prepared for the strumous disease. I am rather, I

am afraid, wandering from the true subject of discussion, and detaining you too long. Let me pass to the question of treatment, leaving it for others to consider the diagnosis to which Mr Annandale has referred. And it seems to me a by no means difficult diagnosis, except in early cases, where the difficulty of diagnosing between strumous glands and lymphadenoma is often great, and must largely depend on the history of the patient. Let me proceed to the matter of treatment. Here, I think, we ought always to comfort ourselves and increase our activity by remembering that the warfare between the bacillus and the tissue of the body is an exceedingly close one, in which very little interference on our part may determine the victory to one side or the other. With that view, you go with greater comfort and decision to the treatment of the case. You may lay down two general principles which are applicable to all inflammatory and tuberculous disease. First, that the treatment must be etiological, must consist in the removal of the cause; and, second, what has been recognised from all time, but more especially since Mr Hilton's book was published, that rest is a universal treatment in all inflammatory affections. Well, then, as to the removal of the cause, How may you remove the cause? First, you will, of course, remove the subsidiary or local cause, and, in this respect, you will bear in mind the extreme importance of removing or preventing septicity. Next in the etiological treatment of the case comes the removal of the tissues affected. And this may be done in two ways—either by excision or by scraping, as Mr Annandale pointed out. The method by excision appears to me applicable to a comparatively small number of cases—to those, perhaps, in which a single gland is affected, is easily removable, and which, without progressing rapidly or being stationary, has yet resisted other methods of treatment. It is right, I have no doubt, in these cases to remove it as a possible source of future general infection. When the glands are more numerous, the scar that you produce, the matting that generally exists, the impossibility of tracing out the many glands that you did not recognise from the outside make excision a comparatively rare resource. Scraping, however, is undoubtedly most useful, always to be employed when you open a gland, and most valuable when the gland is already open, with great certainty enabling you to remove the septicity. I do not think you remove the whole tubercle, but it has to be remembered that quantity is an element of great importance in the warfare, and that, Providence being here, as elsewhere, on the side of the biggest battalions, if you take away so many bacilli, you may place the victory on the side of the tissues. Third, in the etiological treatment you have the possibility of destroying the bacillus; and I think Mr Annandale has been rather contemptuous with reference to injection. The injection of iodine, I am sure, has given good results before suppuration. I have tried

the bichloride injection. I have certainly seen glands that resisted other treatment improve or disappear; and this result would be remarkable if they were simply adenitis. I am quite sure that the iodine internally is a great deal better than externally. In adenitis of a chronic character, the result of local irritation, counter-irritation may do good; but when you have a tubercle bacillus, how can you expect by means of blistering or iodine to cure it? Blistering, iodine, and so on, may, however, do good after the glands have opened, by the painting of iodine well over the interior or pushing the blistering fluid under the skin; and I believe it does so by proving inimical to the bacillus. Passing from these various means of treating etiologically, we come to the treatment by rest, and this is, I think, a method of treatment which has been too much neglected. I have the greatest possible faith in it. It must be complete, absolute, just as you would make it for the spine, the knee-joint, or any other case of tuberculous disease. You must put the neck and the head at rest; and in order to do that I have tried many things, Mr Fleming's bags for the treatment of cervical caries, and an invention of my own. But I have come to the conclusion that the only thing that really is very nice, and does what you want, is the use of some sort of cuirass, with a good stock running up from it and catching by the jaw and the occiput so as to hold the head steady. Mr Bryant the other day had a communication on the subject in the *Lancet*, with an elaborate drawing of what we have been using for years. The helmet and cuirass form an exceedingly useful apparatus for the treatment of these glands in every stage, and in the earlier stages they will frequently cure with rapidity. If the patient is not a good soil, rest will do; even if he is, it will have a remarkably good effect. Rest, then, is a matter of great importance. Let me sum up the methods of treatment which I think ought to be recommended. First, the glands being in the early stage, you apply all those internal hygienic remedies which the physicians present will point out to you. This is most important. You apply also rest by means of a well-padded stock and cuirass. Supposing it has further advanced but not suppurated, then I should use the injection of iodine—two, three, or four drops injected into each gland, according to circumstances. I think the iodine is better than the bichloride. I tried igni-puncture; and I should recommend that, if any one does try igni-puncture, they use that which I used long ago, putting in the platinum wire through the gland, and threading upon it hollow needles, taking care not to heat the hollow needles and hurt the skin. You can then destroy large portions by electric heat without making a skin wound. I must say that igni-puncture has not been so satisfactory as I should have expected, although I have seen cases in which considerable improvement followed, whether because I at the same time put at rest or not I am not quite sure. Suppose the iodine injection fail, then opening and scraping.

I should add to the scraping the application of parasiticides. I myself use salicylic or boracic powder for the purpose of destroying the bacilli, whether pyogenic or tubercular. And, lastly, in a few and limited number of cases, excision of the glands.

Dr Cathcart.—I merely intended to speak on one subject, the local causes of tubercular glands. When I was thinking over the subject I did not expect that the question of the lymph origin of the tubercular bacillus in the glands would be discussed, but would be taken for granted, and therefore, perhaps, I may say a few words as to why I would incline to support the lymph entrance theory rather than that by the blood stream. In the first place, it seems so easy to trace a distinctly local cause in the majority of cases of enlarged glands that it is almost going out of the way to account for it by a condition of the blood. Take, for instance, the enlargement of the glands in the neck from other causes than tuberculosis, as we find them, for instance, in malignant disease. We find the glands distinctly enlarged, and we always attribute that to an involvement of the glands from the diseased locality. In this way, when the disease is in the tongue or jaw, we find the corresponding glands enlarged, and never suppose for a moment that the origin has been an infection going from the original seat into the general system, and afterwards localizing itself in these particular glands. One of the difficulties suggested was to trace the course of the lymph infection. Dr Duncan said that in a large number of cases no visible breach of surface could be ascertained, and also, as the result of his experience, that the glandular affection in the neck was very often symmetrical. In regard to the first of these statements, Dr Duncan, besides stating that he was unable to trace any cause, said, "We find distinctly tubercular manifestations in joints where we cannot find any localized cause which would be the starting-point in them." But Dr Woodhead's investigations throw light upon this, for he showed that in a large number of children dying from other diseases, where there was no suspicion of enlargement of the mesenteric glands, this did exist, and it might account for the condition in joints in such cases as Dr Duncan referred to.

As regards the want of being able to trace a distinct breach of surface, it is not necessary that one should trace the breach of surface at the moment that the glands are enlarged. It is quite easy to suppose that there has been a breach of surface, that the tubercle bacillus has got into those glands, and that the breach of surface has healed, so that at the moment it is inspected there is nothing visible. Dr Woodhead shows that in a certain proportion of cases of enlarged mesenteric glands we are not able to trace injury in the intestine, although that was believed to be the source of them. In my comparatively limited experience, I

have seen a large number of cases where the glands were distinctly unsymmetrical, and have always been able to associate these with want of symmetry in a local origin. The last case was in Dr Duncan's Wards, where he was treating it. The history of the boy was that he had one of his tonsils excised for enlargement some time ago; the other tonsil is now in the same condition, and the glands of the neck are enlarged on the same side as the tonsil involved. Then another point is—If the blood channel and not the lymph channel is the way by which the tubercle bacillus affects the glands, why should it be that these particular lymphatic glands were the first affected? It is quite easy to say, and it would be difficult to gainsay it, that we cannot account for the predisposition to attack certain parts, and that the tubercle bacillus is specially liable to attack glands. But why should it specially be the glands in connexion with the alimentary canal rather than the lymphatic glands of other parts? Treves has shown that in 155 cases of tubercular glands in the Hospital for Scrofula at Margate, something like 133 were in the neighbourhood of the neck, and a small portion connected with the skin, with the joints, in the groin, and the glands in the axilla.

Having thus tried to establish what seemed to be a starting-point, that we must, in most cases, look for a local origin as the causation of enlarged glands, I had intended to limit my remarks to one local cause which was very frequently omitted—that was painless enlargements of the fauces as causes of enlarged glands. The conditions are, first of all, chronic enlargement of the tonsils and adenoid tissue of pharynx, with thickening and ulcerations of the mucous membrane—this condition following a chill or coming on without any other apparent cause, except that the young person is slightly out of health. In two or three cases, seen lately, I found enlarged glands in the neck; and on examining the condition of the pharynx I have discovered, and unexpectedly to the patients, that they were suffering from this condition of the throat. They were not conscious of having any sore throat, and, without examining, one would have supposed that there was no local origin to account for the mischief. The other condition is what one finds in attacks of measles. It is part of the symptoms of measles that the mucous membrane of the nose and naso-pharynx should be attacked, but I think there are probably a large number of cases where the symptoms are so slight that the physician never takes any heed of them, and these are the cases where very often the child recovers from measles but remains with a scrofulous tendency. In a certain number of cases the tubercular attack comes through the alimentary canal,—where it is said that a child was quite well until it had measles. From that we can often trace a distinct tubercular condition of the joints and bones.

As regards the explanation of the pathology of these con-

ditions, it is generally recognised that when lymphoid tissue is attacked the affection of the glands is much more rapid and certain, and that is the reason why it has been found that the glands of the alimentary canal are so frequently involved. The tonsils are themselves masses of lymphoid tissue, and there are localized masses in the pharynx, and all down the alimentary canal there are deposits, especially in the small intestines. What may be the cause of the throat enlargement I am not prepared to say. Whether it is an indication of the strumous taint or hereditary tendency I am not prepared to say, but in most cases under my observation showing glands enlarged and tending to caseate, there was no history which would have accounted for this mischief, and I suppose that it was purely a local condition. Then it is also recognised that any localized injury is liable to promote the entrance of the tubercle bacillus. It is not only a predisposing cause, as Dr Duncan has allowed, but I believe that it is at the same time an actual cause. The tubercle bacillus enters at this weakened spot. Then, as regards the source of the bacillus, Dr Woodhead has given good evidence that the milk is, in many cases, an actual carrier of the tubercle bacillus; and we also know that many tubercular conditions seem to be attributable to the spores found in the air contaminating many parts besides those affected by milk. Thus bronchial glands may be distinctly tubercular. Then, as regards the painlessness of the tubercular glands. One of the distinctive clinical characters of this tubercle bacillus growth seems to me its painlessness. The effect is so slight in itself that unless there be something else working, or at least a very great aggravation of the tubercle bacillus, the irritation it sets up is so slight as not to cause pain. For instance, in strumous joints the patient does not complain of pain until the cartilage is affected. And about tubercular bones there is often no evidence of periostitis. That will help us to understand why the condition in question is so chronic, and why, if there be an actual tubercular condition starting this mischief, it may not be necessarily painful. Then the treatment is partly prophylactic. In the first place, by maintaining the patient's general health we prevent the liability to this condition; but the point I wish to lay special stress upon is that there should be a very careful routine examination of the tonsils and the throat in all cases of enlarged glands, especially those near the angle of the jaw. I have had under my care, several times, patients who have been treated for glands by external applications, in whom I have found a distinct local condition which had been setting up and keeping up the mischief, and without the cure of which any other treatment would be, of course, useless. So much is this the case, that I would also say that it ought to be the routine part of the treatment of measles to examine the throat and then to treat it. I think too much has been spoken in a

general way of treating these conditions. Wherever the throat is in this chronic, painless, enlarged, and ulcerated condition it should be painted with an antiseptic, and I have found that equal parts of tincture of iodine and glycerine locally applied with a brush, combined with absolute rest, a very efficient treatment. I have very often tried the fixation treatment which Dr Duncan has advocated, and found it most admirable. I heard him in one of his clinical lectures laying down this principle, and I have found it most efficient.

Dr James Carmichael.—I am obliged to you for giving me an opportunity of offering a few remarks, and would take leave to say that I think there is no subject which requires more careful attention, both from a medical and surgical point of view, and in which the practitioner requires to avail himself of varied resources. The surgeon who treats the case solely from his own point of view, without due reference to the general condition of the patient, can hardly expect good results. I will confine my remarks to the etiological basis of the treatment, and they will be as short as possible. I believe there is no such thing as idiopathic lympathic glandular disease even in strumous children. The glandular enlargement in all cases is the result of peripheral irritation of skin or mucous surface. It seems to me that the key-note which Mr Annandale struck in the earlier part of his remarks was the right one. We should rather, instead of classing these enlargements of the glands as "strumous," use the term adenitis, because I believe clinically, at all events, we cannot at first distinguish between simple and strumous enlargement of glands. There is no definite, no hard and fast line between the ordinary inflammatory conditions of a chronic nature and struma. We know that in struma the ordinary manifestations are of a chronic nature; that there is no healthy suppuration, but a tendency to formation of cheesy products. The leading features of a strumous gland are these,—that the gland is in a chronic condition; that there is an increase of corpuscular and lymphoid cell formation, with tendency to degeneration, softening, and caseation. In other cases we have a different course, attended with acute inflammation and a formation of pus. I do not think we can always distinguish between the simple adenitis and the tubercular form. In regard to treatment, it must necessarily be etiologically determined, and that is our difficulty at starting. Take a very common example of adenitis—that of the femoral glands from a wound in the foot. We have all seen cases of simple and scrofulous adenitis in this way. A boy running about barefooted gets adenitis of the femoral glands from a wound in the foot. We meet in one case with an inflamed bubo and the formation of a healthy pus which heals under ordinary treatment; in another you have a chronic condition in which you have not the formation of a

healthy pus, but of cheesy products, with chronic inflammation. Mr Cathcart alluded to cases of cervical adenitis in connexion with the faucial and nasal conditions after measles. In his experience in this disease adenitis was much more common in the bronchial than in the cervical glands. The cervical condition is much more common in scarlatina. Bronchial adenopathy is a dangerous complication after measles and also whooping-cough. We cannot doubt that in all these cases we have an irritated condition—a hyperplasia—of the bronchial glands. If the child convalesces favourably, and is naturally healthy, everything goes well. If it is a strumous child, and has a disposition which fulfils those conditions known as struma, then the glands become tubercular or strumous, which I take it, in the present state of our knowledge, are synonymous terms, and the probable ultimate result will be general tubercular infection. There is no doubt that you have these cases of simple adenitis in measles and whooping-cough, because the enlargement of the glands gets perfectly well, and it depends very much on the condition of the child whether this is the case, or the glands go on to tubercular degeneration. Then in regard to the treatment, it must be etiological. You must seek out the cause or you are groping in the dark, and you have no chance of treating the case successfully. All sources of peripheral irritation must be removed. In regard to whooping-cough and measles, I am in the habit of always teaching and advising that children suffering from these diseases should be guarded very carefully during convalescence, because it is often months before the signs of tubercular or strumous disease manifest themselves. Therefore I think the constitutional element is of great importance. The child should be carefully nourished, and should have change of air if possible, and the administration of tonics. But when I say carefully nourished, I would enter here a caveat. We are all too much in the habit of recommending a sort of routine treatment. We say, "The child must have the best nourishment," etc. That is all very well, but if you over-feed a delicate child, it is perhaps worse than giving it too little, because the digestive power is overtaxed. Overdosing with cod-liver oil is a common mistake. Only the other day I saw a little boy suffering from adenitis in the neck, several abscesses had been opened, and I recommended that scraping should be adopted, and the little fellow improved very much; but in this case there was another, and a much more important part of the treatment, requiring attention. He was being stuffed with everything he could take, and the stomach and bowels were in a state of irritative catarrh. I carefully ordered a regulation of the diet, reduced it very much indeed, and stopped all the tonics, substituted simple stomachic remedies, the child improved, and is now doing well. In regard to the mouth, I quite agree with the remarks of the previous speakers as to the importance of attention to it, which is too often overlooked. The slightest

cause of irritation will give rise to enlarged glands—not only give rise to enlargement, but keep it up. It is no use looking at the mouth and seeing that there is nothing about the tonsils and the fauces. You must ascertain the state of the tongue and digestive system generally, and examine all the signs and symptoms, and you will seldom fail to relieve. As to the special surgical treatment, I have perhaps no special title to offer any remarks. Scooping is the best method, I believe, of treating those glands; they seldom require excision except in isolated cases. As regards internal remedies, they are entirely secondary to dietetic and hygienic conditions, but such drugs as arsenic, barium salts, and lime salts are often useful. The chloride of calcium has a wonderful effect on the nutrition of the lymphatic glands. Iron is also beneficial, but must be carefully given. Cod-liver oil is extremely useful, but should be given in carefully regulated doses. External applications of an irritant nature are worse than useless in strumous adenitis. One form of local application, however, the mercurial, is decidedly useful.

Dr Woodhead.—In the few minutes at my disposal, I should like to take up one or two points that have been traversed already by several of the speakers. I should like it to be understood that any supposed pathological fact that does not agree with the clinical history of a case must be thrown on one side. In the same way I should reject all clinical observations that do not agree with pathological facts. First of all, I should like to say a few words about the tubercular inflammation of glands and about the histological changes which may occur in these glands, and to point out that in these changes a giant cell is not an essential part of the tubercular nodule. It is rather of accidental occurrence, not anything specially belonging to it. One finds in the case of tubercular inflammation that this form of histological structure is met with, but it is the result of several conditions into which I need not here enter. There are other forms of evidence of tubercular inflammation quite apart from the giant cell. I should join issue with those gentlemen who look upon the terms used in the classification of glands as all coming under one heading. Undoubtedly they are all forms of adenitis, certain forms of inflammation of the gland; but we must remember that a gland may be enlarged as the result of various forms of inflammation, and that these have a definite course in each case. We know that in certain forms of simple adenitis we have the inflammatory products running through a certain series of changes, and ultimately there may be a return to something very near the normal condition. We have again a form of inflammation in which, owing to the presence of a certain irritant material, certain forms of micro-organisms probably, we have a suppurative inflammation that results in the tissues breaking down (under the action of those micro-organisms). Then, again, as in tubercular inflamma-

tion, we have a special tendency to caseation in certain forms of inflammation. It is this special tendency to caseation in certain forms of tubercular disease with which we ought to deal when we are speaking of struma. It was remarked that in the history of struma we had the history of tuberculosis. That is true so far, but only so far, because struma is only the evidence of one form of tubercular inflammation, in tissues in a certain condition, and with a certain form of irritant. In considering the condition of tuberculosis of glands, we have two factors to take into account. We have, first, the amount and intensity of the irritant material present; and, secondly, the condition of the tissues themselves.

Now, in struma we have a condition in which the gland is exceedingly ready to undergo caseous degeneration in the presence of a small number of bacilli. That is, we have in struma a weakened condition of the adenoid tissue, which causes it to be much more readily affected by the tubercular irritant, and it breaks down under this irritant inflammation, and undergoes caseation much more readily than does a healthy gland. I believe that that is really what we should understand by struma. This condition, of course, may be inherited, or it may be acquired, and I pointed out in a paper I recently published that an acquired condition is very frequently met with in children as a result, say, of catarrhal inflammation of the intestine. The gland itself in non-specific inflammatory conditions has more work to do; its vitality is thereby lowered, and when attacked by the tubercular bacillus, it undergoes inflammatory and caseous changes much more readily than it would if the tubercular bacillus had got in under ordinary conditions. We may have the same condition inherited. Take any tissue in any part of the body in one individual coming into the world under certain conditions. Those tissues will not react to stimuli in the same way as do those in a healthy individual. There is one point, however, that I should like to mention, and that is that we are always apt to go to extremes, and I believe that a very large number of the conditions which clinically are attributed to the presence of tubercle bacilli are actually not due to tuberculous action at all. Recently we have got into the way of assuming that every induration, especially in glands, is the result of a tuberculous process. I for one do not believe this. I am as firm a believer in the activity of the tubercle bacillus as any one, but I don't believe that all the cases put down as tubercular come under that heading by any means. That by the way. There is one point I should just mention here, and that is that Prof. Annandale would keep out the syphilitic inflammation from those cases of adenitis. I believe the syphilitic form is just as true a form of adenitis as is the tubercular. It is one that plays a very important part in a very large number of cases of disease of the glands. Then there has been mentioned a point in connexion with the spread or affection of tubercular glands, whether it is by the lymphatic or the vascular

channels. Dr Duncan said he believed that in a large number of cases the affection of the glands was by means of the blood-vessels. Now, if Dr Duncan means by that the lumen of the bloodvessels, I join issue immediately. If he mean, however, the lymph sheaths around the vessel, and at the terminal ramifications of the vessel, which are more readily affected, then I agree with him. But beyond that I cannot go. In the majority of cases, tubercular affection of the gland, especially the strumous form, is a comparatively chronic process. You find it associated, in almost every case, with tubercular ulceration, because tubercle is essentially a disease of free surfaces. As pointed out by Coats, you always find tubercular ulceration, or the absorption of tubercle from a free surface, somewhere in the neighbourhood of the enlarged glands. In tubercle of the mesenteric glands you may find no ulceration of the intestine, but, as a rule, you can find some history of catarrhal process, or of some irritation of the intestine, in the region of the glands. In the mediastinum you find them associated with tubercle in the abdominal cavity, or connected with disease of the lung—in fact, you will always find evidence of there having been at some period or other an altered surface from which these tubercle bacilli could find access. Then, too, one remembers that the blood supply from these parts to the glands is not a direct one, but that the lymph supply is essentially direct. We may look upon the lymphatic glands as regular filters placed at intervals along the crown of the lymph stream throughout the whole of the body; in evidence of this we find that wherever a tuberculous gland is present the glands beyond it are not affected for some time; once excise the gland, and you find that the second is very rapidly affected. There is a filtering process going on along the course of the lymph stream and along the line of the lymphatic glands. You do not find caseation along a line of glands unless it takes place in a regular order, from one extremity to the other. Then as regards the resistance of the tissues: I have said before this is one of the factors; but another factor is the number of bacilli, because one finds that, in certain cases, there may be a very considerable number of tubercular bacilli actually passing along a healthy gland, and there being destroyed as they pass from the ulcerating surface. But if the gland tissues be weak, then the cells of the gland are not in a fit condition to enter into that contest of which Dr Duncan spoke, and the tissues undergo a rapid degeneration. I think, in connexion with this, one should mention that certain organs appear to be specially affected by bacilli getting into the blood stream. Bacilli may get into the blood stream in a variety of ways, but it has only lately been pointed out that they pass from the lymphatic glands by the blood stream. One finds that if bacilli make their way into the blood stream, there ensues a condition of acute general miliary tuberculosis, and it is only when the bacilli do get into the blood stream that you get a

miliary tuberculosis. It has been suggested that the meningitis of the brain one finds in young children is due to the passage of the tubercular bacilli by the blood channels to the base of the brain. In the same way it has been pointed out that bacilli may pass from tubercular glands into the general circulation, and set up a somewhat chronic form of miliary tuberculosis; where there is a slow passage, not as in the case of hæmorrhage, but a somewhat slower process, as in certain cases of miliary tubercle of a more chronic character, there is still evidence of an undoubted miliary tubercular condition.

Dr Duncan spoke especially of tubercular condition of bones and joints. I believe that in a very large number of cases, and experiments support this fact, cases of tubercular disease specially affecting the extremities of the bones and the joint itself are the result of the passage of the tubercular bacilli along the blood-vessels. But, on the other hand, if one examine a number of the vertebræ in which you have caries, one is able to determine the passage from the glands in the mediastinum to the bone, and thence to the membranes of the cord. It did not appear that there was the very slightest evidence of the passage by the bloodvessels, but there was evidence of the passage of bacilli by the lymphatic channels; but this is in case of the vertebræ, and not in the long bones. In them I believe, with Dr Duncan, that the affection by the bloodvessels is of far more frequent occurrence than the spreading by the lymphatic circulation.

Another point that makes me think that these glands are infected specially by the lymphatic channels is, as we have said, that it is a comparatively chronic condition. Wherever it has been traced to infection by bloodvessels it has been an acute, or comparatively acute condition. In certain glandular cases it is undoubtedly so. It may affect the glands just as other organs may be so affected, but in the majority of cases it is a chronic process. Even where caseation has become advanced, we find there is a dense fibrous layer at the periphery of the caseous mass. This is another point in favour of supposing that the lymphatic glands are affected by the lymph channels. In regard to surgical interference, I think there are certain points which some of the points I have mentioned may help to clear up. From my own observations—and, of course, I do not know how far that is borne out by clinical experience—I should be very chary of scraping without first applying the cautery. I believe in a number of cases the history of an acute miliary tuberculosis dates from the scraping of some of these glands. This we can understand if we remember the condition under which we are working. We have a large, or it may be a small number of tubercular bacilli, a caseous mass in which there are spores, and we have this mass in a closed cavity which we constantly bring into connexion with the circulation by scraping out part of the caseous material and by breaking down the fibrous walls themselves. This strikes me as being

exactly what takes place in hæmorrhage from the lung in phthisis, where you have the presence of small blood-clots in which tubercular bacilli can develop in enormous numbers; unless you can so stimulate the tissues around these clots that you set up a rapid inflammatory reaction, you have the bacilli taken direct into the blood circulation and giving rise to the many tubercular sequelæ. The cautery, I believe, would be of more value than any antiseptic. Iodine would be valuable with the cautery. In iodine you practically set up a granulating surface, formed in the most rapid way you can form it, and to a certain extent protect the bloodvessels from the invasion of the bacilli directly. There are a number of ways, of course, in which this may be done. I believe if you could cause suppuration in a gland by injecting a strong escharotic, you could obtain the same results if you leave the opening free; but in any condition where we have a closed cavity I believe it is dangerous to the health of the patient. I should object very strongly to the injection of bichloride of mercury, which may so weaken the tissues not actually destroyed, that by the time they have recuperated the bacilli have attained the upper hand. Not only that, but the bichloride of mercury in presence of albumen of almost any kind rapidly loses its action as a germicide, and would therefore affect the cells directly in the first instance, and would not affect the bacilli, as one is led to expect, in the second instance. Of course, I am now speaking theoretically, and must ask indulgence from the surgeons, but I give what I believe to be the result of observation. I have noted several other points which I think would be better taken up by other speakers. The only point I should like to insist on is, that in all cases, in the first instance, tubercular glands are localized to that area presided over by them, in which we have the tuberculous irritation or inflammation, and that the process is, in the first instance, the same as in the case of all other irritations; that the irritant is specific in its character, as are also the results.

Dr Woodhead, in reply to a question by Dr Allan Jamieson, said —The chloride of zinc would be valuable to a certain extent if we scrape out the glands and get everything away as freely as possible, but only under those conditions should I say that any antiseptic is of any good at all.

Professor Annandale.—Dr Woodhead has expressed the opinion that there are many cases which resemble tuberculosis but are not tubercular, and mentioned that the giant cell is not necessarily diagnostic of tubercle. Will he kindly tell me what is diagnostic of the tubercular glands? Is it the presence of the bacillus, or the presence of the giant cells, or all of them together?

Dr Woodhead.—I should certainly say the whole of them together. The ultimate result of tubercular inflammation is caseation. In chronic cases, even where caseation is not distinctly

marked, there are almost invariably minute foci of caseous degeneration. In the earlier stages you must look for bacilli, though you may not find them in every stage, yet you do get them in some stage of the disease.

Professor Chiene.—I had intended to make some remarks, but the point that I really wanted to bring out has been brought out by Dr Woodhead. He does not need to crave my indulgence as a surgeon at all. He has put forward exactly the position I intended to take up to-night. I brought two microscopic specimens to bring out the point that scraping tubercular glands is a very dangerous process. The tubercular bacilli are, as these specimens will show, close to the capsule, and I don't believe that it is possible to remove the organisms by the scraping process. I have, I acknowledge, frequently adopted it as a surgeon. Sometimes you are driven to it when you have to deal with an open gland, and the great point, therefore, is to have a free opening. I wish to make one other remark—that bichloride of mercury is not a good thing. It acts upon the cells and destroys them. Recently a series of experiments have been made in regard to a new antiseptic —new in this country, but used in America—a material termed hydronaphthol, which is more powerful than the bichloride. It doesn't act upon the cell elements nearly to the same extent as bichloride of mercury, and I want to bring it forward as an antiseptic. We may be able to introduce it into a cavity after we have found it necessary to scrape the glands. I am pleased to hear that Dr Woodhead desires us to throw aside bichloride of mercury, because I believe it has not been efficacious. The scraping has this tremendous disadvantage, that you open a large number of vessels and expose individuals to miliary tuberculosis. In two cases lately in which I have done this in the prostate (I didn't know then of hydronaphthol), the one patient is dying, and I don't think there is very much chance for the other, as the result of my operations. It is such cases as these that have brought before me the evil of scraping what I believe a primary tubercular gland, by which I mean any chronically inflamed gland. You should excise it, and the sooner the better. You say, "Then you would excise chronic adenitis." I say, "That is a very small matter in the case of a thing which at any time may give rise to disease." I do not believe in the injection of iodine into a gland. You enclose dead tissue, and give the tubercle bacillus a chance. You are to scrape with a large opening that the mischief may escape. All antiseptics have failed me in the past; but hydronaphthol should receive a trial, because, with all respect to Dr Woodhead's views as to the cautery, deep glands in the neck are in a situation which renders it impossible to apply the cautery. I am glad that Dr Woodhead has given his sanction to the fact that scraping should be avoided.

Dr James Ritchie, from a practical point of view, thought it of great importance that the relation between struma and tubercle should be recognised. He believes that in struma we have simply a peculiar vulnerability of constitution which may be inherited or acquired, syphilis playing a very important part in some cases. But that doubtless there are many cases in which, through unfavourable hygienic conditions, or overfeeding, or something which has injured the constitution of the individual, he is made much more liable to the invasion of the tubercle bacillus.

Meeting IX.—May 1, 1889.

Professor CHIENE, *Vice-President, in the Chair.*

I. DEATH OF AN HONORARY MEMBER.

The President read a letter from Madame Donders intimating the death of her husband, Professor Donders of Utrecht. It was unanimously resolved to enter it in the minutes and send a message of sympathy to Madame Donders.

II. EXHIBITION OF PATIENT.

Mr F. M. Caird showed a PATIENT on whom the operation of skin-grafting had been performed for an extensive destruction of the skin of the left upper arm by burning. The upper portion of the sore had been treated first by Thiersch's method, large pieces of epidermis being transplanted from the patient's thigh with a very successful result. The lower portion was afterwards treated by grafting of small pieces taken from a recently amputated foot. Two coloured drawings showed the sore in different stages after the skin-grafting.

III. EXHIBITION OF DRAWINGS.

Mr Caird showed COLOURED DRAWINGS OF SYMMETRICAL GANGRENE OF THE EXTREMITIES. The patient was a woman. She had had no premonitory symptoms such as occurred in Raynaud's disease.

IV. ORIGINAL COMMUNICATION.

PLEURISY AND PLEURITIC EFFUSION FROM THE ANATOMICO-PHYSIOLOGICAL STANDPOINT.

By ALEXANDER JAMES, M.D., F.R.C.P. Ed., Assistant Physician, Royal Infirmary, Edinburgh; Lecturer on the Practice of Physic, School of Medicine, Edinburgh.

IN whatever way induced, the essential changes from the normal in pleurisy are hyperæmia with infiltration of the serous and sub-

serous tissue, proliferation of the epithelium and of the protoplasmic connective tissue corpuscles, and effusion of inflammatory lymph, all resulting in the formation of an inflammatory false membrane on the pleural surface, and the collection of an inflammatory fluid in the pleural cavity. As this fluid increases in amount space for it is made mainly by the collapse of the corresponding lung, the inflamed pleural surfaces being thus separated. Sooner or later the inflammatory process terminates, and the effusion of inflammatory lymph ceases. Reabsorption of the fluid then occurs, in part through the bloodvessels, and in part through the lymphatics, the numerous entrances to which in the form of stomata exist upon the costal, and possibly also upon the pulmonary and diaphragmatic pleuræ. With the reabsorption of the fluid the lung expands. In favourable cases the pulmonary and parietal pleuræ are soon again in close contact, the thin, or perhaps rather the thinned, layer of false membrane between them becoming organized, and serving to form more or less firm adhesions. In less favourable cases the processes of fluid absorption and lung expansion do not work together so smoothly and uninterruptedly. As the result of delay in the fluid absorption process, allowing the lung to be bound down by firm, organized bands or layers of false membrane, the absorption of fluid is not accompanied by a corresponding expansion of the lung. There results, therefore, (1), a drawing together of the surrounding parts, namely, chest-wall, mediastinum, and diaphragm; (2), a thickening and fibrosis of the pleuræ and false membrane; and (3), with a fibrosis of corresponding portions of the lung substance itself, a traction upon it outwards so as to produce dilatations—fusiform, saccular, or irregular—of the bronchial tubes.

The degree to which all these morbid processes are manifested varies within wide limits, but this does not concern us at present. What does concern us is as to how they are explained by anatomy and physiology, and as the above epitome of the pathology of pleurisy and effusion resolves itself into three parts, namely, pleural inflammation, fluid effusion, and fluid reabsorption, it will be most convenient to consider the data which the anatomy and physiology of the lungs afford in connexion with each of these.

The first point, therefore, which seems to require notice is the *locality* or *site* at which the pleuritic process is most prone to manifest itself. In the vast majority of cases of so-called idiopathic pleurisy this is in the lower and lateral region. As the patient says the pain is in his side, and it is here that the characteristic friction sound is to be heard most distinctly and loudly. Of course, in secondary pleurisies the locality may be very different, but this can be easily explained. Thus, tubercular deposit at the lung apex readily involves the pleura and causes an apical pleurisy, and the great frequency of adhesions at the apices and posterior borders of the upper part of the lungs corresponds with the tendency of phthisical lung disease to manifest itself at the apices and

extend along the posterior borders. Farther, the virus from a tubercular peritonitis may pass through the diaphragm and cause a diaphragmatic pleurisy, and malignant disease of the glands of the neck, of the glands of the axilla, or of the mammary gland, readily involves those parts of the pleura which are in closest proximity. At present, however, we have to do with so-called idiopathic pleurisy, and the question again is, Why should it be most common at the lower and lateral regions of the chest?

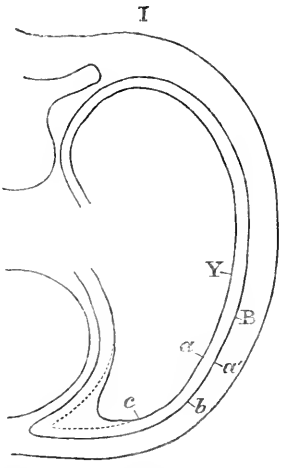
One explanation is, that it is here that the chest-wall is thinnest, and consequently the great irritant cold will best be able to exercise its injurious effect on the underlying pleura. Certainly all of us who have daily opportunities of examining chests can understand the importance of this explanation, for we know that this is, even in not very well nourished individuals, the only place where ribs and interspaces are not rendered invisible by overlying muscle. Against this theory, however, it may not unjustifiably be argued, (1), that the effects of cold are not necessarily manifested at the locality exposed; (2), that inflammation of another serous surface, the peritoneum, can seldom be ascribed to cold; and (3), that in women, in whom the acknowledged evils of low-bodied dresses have to be considered, pleurisies show no special tendency to affect the upper part of the chest. Still the theory is one which deserves notice.

But a consideration of the movement of the lung in the pleural cavity gives us further information on this question of locality. Thus, if in a living animal pleural windows be made, *i.e.*, if small portions of skin, fascia, and intercostal muscle be removed at different parts of the chest, so that through the transparent costal pleura the movement of the pulmonary pleura can be demonstrated, it will be seen that the amount of this movement is greatest at the lower and lateral regions, and becomes gradually less and less as we pass upwards or backwards, so that the apex and posterior border of a lung are practically fixed points. Hence the part of the pleura where symptoms and physical signs show pleurisy to be most common is the part of it where this movement of the pulmonary against the costal pleura is most marked; and it may be suggested that the correspondingly great stretching and relaxing of the pleural membrane which this implies will render it at this locality specially liable to be affected by morbific influences.

What is probably, however, of more importance in connexion with our efforts to explain why pleurisy seems to be specially prevalent at this lower and lateral region of the chest is the fact that, as the result of the greatest movement being at this locality, a pleuritic process once originated will here have a special tendency to be rendered more severe.

This has been well shown by Rindfleisch, and can easily be understood from Diagram I. This, which represents a section through the chest about the level of the sixth rib, shows the

lung in the position of expiration, the area included by the dotted line representing its increase in inspiration. Suppose that at the point *a* on the pulmonary pleura we have a focus of inflammation. During expiration it will be opposite a^1 on the costal pleura, and will infect it at that point, but during inspiration it will be moved forward, and will infect the costal pleura at *b*, and with the next expiration a portion of pulmonary pleura further forward will be opposite to *b*. This in turn will be infected, and with the next inspiration the costal pleura opposite *c* will be involved. In time the pulmonary pleura at *c* will become affected, and so with the respiratory movements the inflammatory process will be transmitted forwards. Similarly, it will be transmitted backwards, B, Y being with the respiratory gliding of pulmonary and costal pleuræ successively affected.



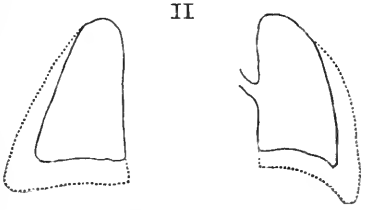
Seeing, then, that this movement spreads the pleuritic process, it follows that where it takes place to the greatest extent the pleuritic process must be most marked and severe. A small focus of pleural irritation, which at any part of the apex or posterior border of the lung would be able to induce but slight irritation and inflammation, has much greater potency for harm at any part of the lower and lateral regions.

The great proneness, therefore, of pleurisy to affect the lower and lateral portions of the lung may be thus ascribed to the great amount of respiratory expansion and recoil rendering the parts there more vulnerable, and tending to a correspondingly great extent to spread and aggravate any morbid change in the pleura which may have occurred. Conversely, the fact that in a phthisical lung the pleuritic adhesions almost always present are so limited to the parts most affected by tubercular disease, *i.e.*, to the apices and posterior borders of the upper part, and show little tendency to spread, may be ascribed to the very slight amount of movement of lung in those regions. Farther, it is not unjustifiable to consider that the relative absence of symptoms and physical signs (pain and friction sounds) in such phthisical pleurisies may be similarly explained.

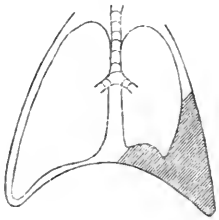
Effusion of Fluid.—As the fluid begins to collect it first fills up the so-called complemental space of the thorax, *i.e.*, the space between the diaphragm and costal wall into which the lung passes at each inspiratory increase. As it accumulates, more and more space is allowed by the progressive collapse of the lung; but now

it is to be remembered that this collapse does not take place to an equal extent in all directions, but that it is most marked in the lower and antero-lateral region, where, as we have just seen, lung movement is greatest. Thus, in Diagram II., which represents a lung looked at laterally, and from the front, the greatest amount of collapse is seen to be from the lateral and lower portion inwards and upwards towards the lung root. Hence, as a lung collapses in the pleural cavity, the space formed by the separation of the pulmonary from the costal and diaphragmatic pleuræ is peculiarly shaped. Looked at on section from in front (Diagram III.), its base is formed by the diaphragm, its outer border by the costal pleura, its apex by the junction of costal pleura and lung, and its inner least regularly shaped border by the lung and mediastinum. Looked at next from the lateral aspect (Diagram IV.), the upper limit of this space, formed by the line of junction of pulmonary and costal pleuræ, is arched, the arch being highest laterally and lowest posteriorly. Hence, then, as fluid collects in the pleural cavity, its surface is not a level one like that of fluid in a jar, but a curved one.

II



III



IV

This curved line was well demonstrated by Garland of New York, by injecting warm fluid cacao butter into the pleural cavities of animals and of man after death. On opening the pleura after the cacao butter had cooled and solidified, he found its upper surface curved as described.

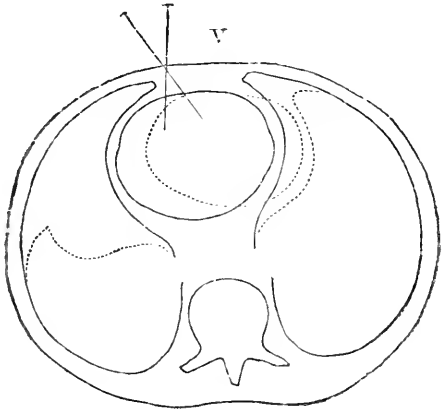
Garland expressly states that in his experience, "even with injections which filled one-half of the pleural cavity there was little or no intrusion of the fluid between the lung and chest wall, but the model all lay between the lung and the diaphragm." Post-mortem examination in pleurisy, however, will often reveal a condition more like that depicted in Diagram III. where this intrusion has occurred. This curved line can also be demonstrated by percussion in disease. Thus, in pleurisy, whilst effusion is going on, it may be recognised, more easily, however, in hydro-

thorax with the patient in a sitting posture, and probably most easily in pleuritic effusion when reabsorption of the fluid has begun to take place. Of course, for its manifestation, certain conditions must be fulfilled. Thus a certain amount of fluid is required. If the pleural cavity is quite full, the lung is not only collapsed, but is airless and even compressed, and percussion dulness is met with all over. With reabsorption, however, resonance begins to show itself anteriorly and posteriorly, and the curved line is manifested. Gradually, as reabsorption goes on, the arch of the curve gets flatter and flatter, till finally it disappears altogether. Again, in order that this curved line may be demonstrated, there must be no adhesions from previous pleurisies to bind together at this or that point the pulmonary and costal pleuræ, and, of course, the lung tissue itself must be healthy.

Next, in this connexion, the influence of the position of the patient on fluid collections in the pleural cavity requires notice. In hydrothorax, or in the accompanying or ensuing fluid effusions of pneumothorax, this influence is well marked, the percussion dulness caused by the fluid altering its position with that of the patient quite readily. With the fluid of a pleurisy, however, it is different. Here the pulmonary and costal pleuræ tend to adhere wherever they are in contact, and although in the early stages, when the fluid is increasing in amount, its position, as ascertained by percussion, may alter to some extent on altering the position of the patient, in the later stages, when reabsorption is occurring, the adhesion of costal to parietal pleura is so firm that this does not take place. Further, the precise position of a curved line (see Diagram IV.) may vary to some extent according as the patient has been lying in bed or moving about during his illness. In the former case it will, as might be expected, show the highest portion of its arch more posteriorly.

But with the accumulation of fluid and collapse of the lung other changes are associated. These are displacement of the mediastinum, with the heart to the opposite side, and displacement of the diaphragm, with the liver, stomach, etc., downwards. In most of the text-books these displacements are said to be due to pressure of the effused fluid, but this is in many cases an error. As regards the mediastinum and heart, this has been shown very clearly by Douglas Powell. Suppose that on the body after death we push a long needle through the fourth left interspace, so that its point enters the heart, as is shown in Diagram V., which represents a section through the body about the level of the fifth rib. If next we make a free opening into the left pleural cavity, air enters and collapse of the lung occurs; but what is of most importance to notice is, that the heart is displaced to the right side, as indicated by a tilting of the head of the needle to the left. Here nothing has pressed the heart over, it has simply been drawn over by the elastic retraction of the right lung. Under ordi-

nary circumstances the heart and mediastinum are, as it were, held in position by the two lungs drawing them each to its own side: if



this traction is abolished on one side by letting air in, as in Powell's experiment, or by fluid collecting, as in pleurisy or hydrothorax, the heart is displaced towards the other. It will thus be seen that for this displacement pressure need not be existent at all. As regards the diaphragm the case is different. Supported from below by the pressure of the abdominal viscera, its normal arched form is practically independent of lung traction on its upper surface. Hence an accumulation of fluid in a pleural cavity does not tend to lower it as the result of abolishing lung traction, but does so only when the fluid is sufficiently great in amount by its weight, or by causing positive pressure, to press it down. Clinically, the point of importance in connexion with this is, that whereas displacement of the heart and mediastinum occurs with even small accumulations of fluid, and is, therefore, a common and an early sign of its presence, displacement of the diaphragm, which can only occur with considerable effusion, is a less common and a later sign of the disease. On the other hand, it is to be remembered that in prolonged cases, where a lung has been bound down and reabsorption of the fluid is occurring slowly, a traction upwards of the diaphragm is specially apt to take place.

But there are other two clinical points of special importance in connexion with what has just been stated. The first is, that if by means of a manometer the pressure of pleural effusions be tested, it will be found that even with large effusions this pressure is never very great, and that with effusions considerable in amount the pressure may be negative. Thus Homolle states, that the greatest positive tension which he obtained in his cases was 15 mm. Hg., and the greatest negative pressure was 38 mm. Hg. in inspiration, and 33 mm. Hg. in expiration. In empyemas

the positive tension was always greater than in serofibrinous effusions.

The second is, as regards the amount of fluid which can be contained in the pleural cavity. It is evident that this amount must vary with the size, age, etc., of the individual, but in an ordinary adult a pleural cavity may be considered to be full when it contains 100 to 120 ounces. With a quantity of about 60 ounces, percussion dulness will be met with up to the third rib or interspace, and with 35 or 40 ounces up to the sixth rib or interspace. With a quantity of 8 or 10 ounces percussion dulness will be just recognisable about the base or lower axillary region.

Reabsorption of Fluid.—In connexion with the reabsorption of the fluid in pleurisy, Dybkowsky's experiments are, from the anatomico-physiological standpoint, probably of the greatest interest and importance. That observer demonstrated, that if watery solutions of grape sugar or gum arabic, or if water containing in suspension minutely divided particles of Berlin blue, were introduced into the pleural cavity of an animal, these could in time be absorbed. But he also showed, that for the absorption of either of these the performance of respiratory movements was necessary. Thus, if they were injected into the pleura after death, or into the pleura of a living, quietly breathing animal, none or little was absorbed. Injected, on the other hand, into the pleura of a dead animal, whilst powerful artificial respiratory movements were being induced, or into the pleura of a rapidly breathing, living animal, absorption took place readily. Dybkowsky further showed that whatever increased the elasticity of the lungs favoured absorption, for when the trachea of the animal experimented upon was connected with a space in which the air pressure was lowered, absorption was increased, while if it were connected with an air space with higher pressure, it was diminished or arrested.

In the case of the fluid solutions, this absorption seemed to take place as the result of filtration and osmosis through all the surrounding parts,—costal, diaphragmatic, mediastinal, and pulmonary pleuræ,—into the underlying bloodvessels and lymphatics. In the case of the solid particles of Berlin blue, however, it was different. In all his experiments, he found that the absorption of these was limited to the costal pleura, and that there it took place through stomata into the lymphatic vessels. These stomata and lymphatics, he further demonstrated, were limited to the intercostal spaces,—none were found on the portions of pleura corresponding to the ribs.

From these experiments certain important practical deductions can be drawn.

I. That although in the early stages of pleurisy the respiratory movements tend to spread and intensify the pleuritic process, in a later stage they tend to promote reabsorption of the fluid effusion.

II. The fact that the absorption of minute solid particles from

the pleural cavity is brought about by the stomata and lymphatics of the costal pleuræ, is of clinical importance in that when in a case of pleuritic effusion, approximation of the ribs has occurred, these stomata and lymphatics will be rendered correspondingly incapable of performing their function. Further, the mobility of the side being correspondingly impaired, the benefit of the respiratory movements will be to an equal extent awanting.

III. The fact that by breathing an attenuated atmosphere the process of reabsorption is favoured, is of importance in connexion with the ærotherapeutics of pleuritic effusion.

Dr Affleck thought Dr James's paper required more consideration than they could give it immediately after hearing it read. He would refer to only one or two points. The position of the pleurisy was probably in the majority of cases as Dr James had said, but it was by no means always so. He accounted for this position by its greater exposure to cold and the freer movement of lung taking place there. The first of these, exposure to cold, was a cause which could not be ignored. Some constitutional condition, however, very often preceded the action of cold. It was curious to note how seldom a contiguous serous membrane, the peritoneum, was affected by cold. With regard to movement of the chest, Dr James was probably aware that relatively in women the upper part of the chest moved much more than the lower. Had he made any observations on pleurisy in women, bearing this fact in mind? As to the level of the effusion, his experience was that it was frequently highest posteriorly and lowest anteriorly, just the reverse of what Dr James had found. This was a matter governed probably by the exudation and adhesions which had taken place.

Dr James Ritchie agreed with Dr Affleck that the constitutional condition had much to do with the development of pleurisy, and probably after it the causes laid down by Dr James came into play, but he doubted whether the thinner covering of the lower costal parietes had any causal relation. He did not accept Dr James's explanation of slight displacement of the apex beat of the heart in cases of moderate effusion. In the experiment on the dead body described by Dr James in support of his theory, Dr Ritchie believed that the displacement of the heart is chiefly due to atmospheric pressure at the side opened, and not to the elastic traction of the opposite lung, and he believes that all effusions exert pressure in proportion to their amount. As a further proof that fluid caused no intrathoracic pressure, Dr James stated that much fluid might be present and yet fail to flow when a trocar was inserted. Dr Ritchie had never experienced this result, and he believed it could only happen if the lung were bound together by strong adhesions. He wished to know why vocal fremitus was increased when the lung began to expand.

Dr James Carmichael thought the apical physical signs of pleuritic effusion were of great clinical interest, more especially in the case of children, in whom they were specially distinctive. He thought Dr James's explanation of the cause of these apical signs was probably the correct one, and he commended his clear explanation and elucidation of them. He recollected many years ago hearing Traube of Berlin in one of his cliniques give a very similar explanation of the phenomena. He did not agree with the theory that in pneumo-thorax they had always collapse of the lung. There was a certain class of cases in which Nature effected a cure of purulent effusion. He referred particularly to those cases where there was a limited or partial pneumo-thorax established in the efforts of Nature to effect a cure—those cases in which there was a discharge of the pus by the bronchi, and limited pneumonic areas on the surface of the pleura, ulceration of the membrane, and escape of pus into the bronchial tubes. In these cases they had pneumo-thorax present and coincident with this expansion of lung. He had met with many of such cases, the last not many months ago. A little boy expectorated in this way a pint of purulent fluid in a very short time, and a perfect cure resulted. This was a matter of very great interest, showing that clinical facts did not always accord with preconceived ideas on the subject. In ordinary cases of pneumothorax in which a partially or generally consolidated lung existed the tendency was to retraction rather than expansion.

Dr W. Russell said Dr James drew attention to a matter well known to physiologists, but which clinicians were apt to ignore—the greater functional activity of the lower part of the chest. Dr Affleck's reservation on this point he thought correct, for in the lungs of females in the post-mortem room the upper parts were more emphysematous than in males. Whether the greater functional activity of the lung at the lower part was to be regarded as a common cause of pleurisy depended on the question whether this was the commonest situation for pleurisy. His own experience would hardly bear this out. The greater majority of pleuritic adhesions he found post-mortem at the apex and along the posterior aspect of the lung. Upon the determination of the situation hung a considerable part of Dr James's contentions. The process was supposed to spread from this lower surface by infection. He could not say he was inclined to accept this infective theory. The question arose, Why should the action cease before it involved the whole lung? Further, they knew that in inflammatory affections of the pleura in this situation there was great immobility. The diaphragm was restrained from its action, and the chest-wall was also restrained. This might, of course, account for the arrest of the inflammatory process. The statement that the apex did not give symptoms of pleurisy when present was one he was hardly prepared to accept. There was, no doubt, difficulty in distinguishing between a pleurisy at the apex and a pneumonic

condition, but because of the difficulty he was hardly willing to grant that the condition did not give rise to palpable symptoms which the physician might recognise. He was not inclined to accept Dr Powell's and Dr James's views as to the displacement of the heart and diaphragm. It was all very well to talk of the elastic traction of the lung, but it was difficult to understand how this acted, seeing the lung was in a condition of hyper-distention; and, in addition, the view was based on the statement that in moderate degrees of effusion there was displacement of the heart and diaphragm. This was a statement open to question. He doubted if there was such displacement of heart or diaphragm in moderate degrees of effusion. He agreed with Dr Ritchie, for he had found no difficulty in removing fluid from the pleural cavity with an ordinary trocar and canula. The position of the fluid was determined very much by the position of old adhesions. They frequently found effusions limited by old adhesions. The presence of these, and owing to them the compression or collapse of the lung in certain directions, gave rise to physical signs which sometimes were confusing. Not long ago he saw a case in which pneumonia was diagnosed at the posterior part of the lung. The physical signs were dulness on percussion, bronchial breathing, and some fine crepitations. There were certain points connected with the case which led to the belief that there was effusion, and the aspirator drew off a large quantity of fluid, and forthwith the bronchial breathing, increased fremitus, and other signs of pneumonic consolidation disappeared. This was a mistake which he fancied was oftener made than they perhaps were inclined to recognise. The view with regard to the cause of increased fremitus was a feasible one, but there was evidence to show that the carnification of the lung produced necessarily a condition analogous to pneumonic consolidation. The lung was practically a solid body. It acted like a solid body in conducting sound. They had bronchial breathing as in pneumonic consolidation, and the breathing louder than it was over the trachea. The lung was really, then, an intensifier of sound.

Dr James agreed with what had been said of cold as a cause of pleurisy. It was not his own statement. In the books the covering of the lung was said to be thinner here than elsewhere, and this was supposed to influence the situation of the pleurisy. He did not believe that in the natural state the upper part of the lung in women was the most used. He did not believe that pleurisy in them was more frequent at the upper than at the lower part of the lung. As to the level of the fluid, he was well aware that that was very much governed by adhesions and the state of the lung. What he said was that, if they took a healthy lung in a healthy pleura and allowed fluid to collect in that pleural cavity, they got a curved line which was highest laterally and lowest posteriorly. In referring to Douglas Powell's experiment, he wished to explain

that when an opening was made in the pleura the corresponding lung collapsed completely, the opposite one slightly, and the heart movement showed that in the normal state each lung dragged upon the heart to a certain extent. By opening into one pleura one lung collapsed, and its elastic traction on the heart was taken away. The adhesions at the apex of the lung and along its posterior border referred to by Dr Russell were, he held, due to secondary pleurisies. Pain and friction were certainly not marked in these positions as they were at the lower part of the lung. As regards the vocal fremitus, he was glad to know from Dr Carmichael that a similar explanation had been given by Traube. He was not aware of it, and had not found it referred to in any of the books, and had regarded it as original. As to consolidation, Skoda had shown that consolidated lung did not conduct sound better than the healthy lung tissue (*Markham's Translation*, p. 39), hence the increased vocal fremitus and resonance so often met with in the reabsorption stage of pleurisy could not be explained in that way.

Meeting X.—May 15, 1889.

Dr JOHN SMITH, *President, in the Chair.*

I. EXHIBITION OF PATIENTS.

1. *Mr A. G. Miller* showed a young man whose KNEE-JOINT he had excised three years before. The result had been a very successful one, the operation having been done for synovial disease.

2. *Mr F. M. Caird* showed a young man suffering from PARALYSIS OF THE RIGHT SERRATUS MAGNUS. There did not seem to be any special cause for it, and the patient himself said it came on with pain in the shoulder after exposure while driving. The scapula projected in the back whenever he pushed forward against an obstacle. The digitations of the muscle were hardly visible.

II. EXHIBITION OF PATHOLOGICAL SPECIMEN.

Dr Argyll Robertson showed a DARK CATARACT removed from a woman about 65 years of age. Before operation the perception of light was too small to be accounted for by the condition of the lens. It was evident that the interior of the eye was not healthy, and there was found extensive atrophy of the optic nerve and pigmentary degeneration of the choroid. The other eye was operated on afterwards, and was in the same condition. On the whole, the result in both instances was satisfactory. Her vision, which

was so bad that she required to be led about, was so far re-established that she was now able to guide herself with ease and to read good large print.

III. Original Communications.

1. HOSPITAL EXPERIENCES.

Part I.

By John Chiene, F.R.C.S. Ed., F.R.S.E., Professor of Surgery, University of Edinburgh.

At the termination of last winter session (1888–89) eleven years had elapsed since my appointment as Surgeon to the Edinburgh Royal Infirmary. The time had come when it appeared to me that benefit might be derived from a general retrospect of the cases which have been treated in the wards under my charge.

Beginning in the old Infirmary, where I had charge of two small wards for one year, and afterwards in the present Infirmary, first in Wards IV. and V. for three years, and then for seven years in Wards XIII. and XIV., there is much of interest to me personally to look back upon, and perhaps a tabulation of the experience gained may be of use to others.

I wish it to be distinctly understood that I place on record all the cases of which notes have been preserved in the Infirmary records. The great majority of these cases have been under my own care, but I have been ably assisted by the assistant-surgeons, Dr Macgillivray, the late Dr Bishop, Dr Cotterill, Mr Cathcart, and Mr Caird. I have therefore to place on record our combined work. It seems to me that over such an extended period this will be more valuable than if I had made it only a personal record. It will be the only true record of the routine work of a surgical ward as at present conducted in the Edinburgh Royal Infirmary. I am entirely responsible for what I have done, and for the way in which it has been done. Mr Wallace and Mr Robert Robertson have given me much assistance in the tabulation.

I find that notes have been preserved of 2658 cases; of these 1600 are operation cases, noted in the operation book. While I believe that the record of the operations is very accurate, I am of opinion that all the non-operation cases have, especially during the summer holidays, not been carefully noted. I ask it also to be remembered that the clerks and dressers who take the cases are second year students, and I therefore ask some allowance for their inexperience.

The classification which I have followed is the same as in the syllabus of the systematic lectures in the University; it has faults, but at the same time it has advantages. I am best acquainted with it; and although in a clinical retrospect I would have desired a purely regional classification, still I think on the whole it will be

best to adhere to the classification in the syllabus. In its main features it is pathological and anatomical, looking to the organs and tissues. In some cases the basis is regional, as in head injuries, mammary, abdominal, and pelvic cases, which could not properly be included in the primary divisions of the classification.

The cases are made up as follows:—

Inflammation and its results,	340	cases,	24	deaths.
Injuries, . . .	563	„	29	„
Diseases of organs, .	690	„	23	„
Deformities, . .	113	„	1	„
Tumours, . . .	285	„	29	„
Diseases of regions, .	667	„	59	„
	2658	cases,	165	deaths.

The figures may vary slightly as regards the non-fatal cases. As regards the deaths, I believe that they are correct. They are given at present as a general indication of the work done. Their analysis will be work for the future.

In dealing with this mass of material, the first thing that attracted my attention was the deaths, and I purpose to-night directing attention to them. I do not consider them in relation to the successful cases; that subject will, if opportunity offers, occupy our attention on another occasion. I wish to consider these deaths in relation to their causes, and I wish from the experience of eleven years to draw some general conclusions which, while they will directly bear on my own practice and the dangers which I and those who may be associated with me will have to avoid in the future, may indirectly be of service to those who may hear me.

The question at the outset arises—What is the best method to adopt in classifying a series of deaths? The practical answer seems to be—Classify them under the different causes antagonistic to success; in other words, under the different conditions against which the surgeon has to contend in order to avoid death.

Given blood in a healthy condition, and given a healthy nervous system, our patients would not die. We have, then, blood lesions and nerve lesions. The blood lesions have reference to the channels which convey the blood, and to the blood itself. We know that the great cause of blood contamination is accumulation of organisms and their products in the blood-stream. Organismal causes are therefore closely allied to blood lesions.

The condition of exhaustion under which some deaths must be classified seems to me intermediate between organismal causes and nerve lesions. Some of these cases of exhaustion are apparently organismal, some are allied to nervous shock; and, further, the condition termed shock has an intermediate position between nerve lesions and blood lesions, because in surgical shock we have

very often a mixed condition, partly nerve shock and partly shock from loss of blood. We have in this way a cycle. Beginning with blood lesions, we pass from them to organismal causes; from them we pass to exhaustion, from it to nerve lesions, and then we reach shock, which in its turn brings us back to blood lesions—the point where we began.

Under these five heads I find that

7½	per cent.	of the	deaths	are due to blood lesions.
47½	„	„	„	organismal causes.
12	„	„	„	exhaustion.
14	„	„	„	nerve lesions.
19	„	„	„	shock.

Blood Lesions—13 deaths. 7½ per cent.

Seven cases of non-inflammatory gangrene, due to imperfect supply of blood. One of these cases was aged 25, and died of phthisis; the remainder were people above 65 years of age. In one case the arm was affected, in the remainder the foot.

One case of thrombosis following tapping of a cystic goitre. One case of fatty heart in a female with scirrhus tumours in both mammæ. One case of angina pectoris in a man who suffered from strangulated inguinal hernia, which was reduced *en bloc*, and on whom, after relieving the strangulation, the radical cure was performed. Three cases of hæmorrhage—two secondary; one an epithelioma of the neck, in whom the common carotid required to be ligatured; another, a scirrhus of the mamma, in which the glands in the axilla were removed, and in which the axillary artery gave way by ulceration. In both of these cases sepsis was the predisposing condition. One case of reactionary hæmorrhage following excision of a large vascular tumour on the posterior aspect of the thigh.

Organismal Causes—79 cases. 47½ per cent.

Acute organismal fever (sapræmia—septicæmia),	20 cases.
Chronic organismal fever (pyæmia),	13 „
Tuberculosis and tubercular meningitis,	16 „
Erysipelas,	7 „
Tetanus (traumatic),	2 „
Diphtheria,	2 „
Septic pneumonia,	3 „
Septic peritonitis,	4 „
Septic meningitis,	2 „
Surgical kidney,	3 „
Traumatic gangrene,	5 „
Carbuncle,	2 „

These cases may be grouped with advantage under three heads:
First, Those patients who suffered from organismal poisoning before admission—24 cases.

Second, Those patients in whom there was an ulcerated surface on admission—29 cases.

Third, Those patients in whom the skin was unbroken on admission—26 cases.

In the third class we have a series of cases which must be considered as distinct calamities—cases in which antiseptic precautions failed to keep the wound aseptic.

Exhaustion—19 cases. 12 per cent.

The predisposing causes to the deaths were as follows:—

Tumour infection,	6 cases.
Old age (above 80),	2 „
Youth (a case of double hare-lip in a weakly child),	1 „
Chronic bronchitis,	1 „
Œdema of lungs,	1 „
Disease of heart,	1 „
Starvation,	2 „
Carbolic poisoning,	1 „
Persistent vomiting,	1 „
Double operation,	1 „
Loss of blood before admission,	2 „

Nerve Lesions—23 cases. 14 per cent.

Uræmia,	6 cases.
Inflammation of lungs in extensive burns,	2 „
Cardiac syncope,	3 „
Brain injury,	10 „
Rupture of spinal cord,	1 „
Locomotor ataxy,	1 „

Shock—31 cases. 19 per cent.

Accidents,	8 cases.
Burns,	4 „
Tumours,	4 „
(One case of retroperitoneal sarcoma; two cases of rectal tumour; one case of sarcoma of the synovial membrane of the knee-joint.)	
Excision of the knee-joint,	1 „
Radical cure of hernia in a child 16 months old,	1 „
Strangulated hernia,	5 „
(4 of which were moribund on admission.)	
Amputation at the hip-joint,	1 „

Gastrotomy,	2 cases.
Œsophagostomy,	1 „
Laparotomy,	1 „
Anæsthesia—2 of chloroform, 1 of ether,	3 „

One case of death during administration of chloroform was reported by Mr Silke in the *Edinburgh Medical Journal*, vol. xxv., p. 1007. The second case occurred in an epileptic in whom an attempt was made to reduce an old-standing dislocation of the shoulder.

The case in which ether was associated with death was a woman in whom the breast was excised for scirrhus. She died on the third day of acute bronchitis, in all probability due to the administration of ether.

From another point of view these 165 cases may be considered. They may be divided into three distinct groups:—

1. Cases in which disaster was to be expected—85 cases.
2. Cases in which there was a fair hope that disaster would not occur—43 cases.
3. Cases in which unexpected disaster occurred—37 cases.

Under the first head we may include 85 cases.

1. Under organismal causes—		
Blood-poisoning at time of admission,	24	cases.
2. Under exhaustion—		
Severe loss of blood before admission,	2	„
Tumour infection,	6	„
Starvation,	2	„
Old age,	2	„
Lung and heart affections,	3	„
3. Under shock—		
Severe accidents, death within 24 hours,	8	„
Severe burns,	4	„
Strangulated hernia, operation performed too late, bowel gangrenous,	3	„
4. Under bloodvessels—		
Senile gangrene,	7	„
Fatty heart,	1	„
Angina pectoris,	1	„
5. Under nerves—		
Severe injuries to brain, 9 } and spinal cord, 1 }	10	„
Locomotor ataxy,	1	„
Uræmia,	6	„
Cardiac syncope,	3	„
Inflammation of lungs (burns),	2	„
	85	cases.

Under the second head, 43 cases.

Broken skin at time of operation, . .	29 cases.
Laparotomy, ...	2 „
Tumours, ...	3 „
Gastrostomy, ...	2 „
Œsophagostomy, ...	1 „
Hernia, ...	3 „
Thrombosis, ...	1 „
Persistent vomiting, . . .	1 „
Amputation at hip-joint, . . .	1 „
	43 cases.

Under the third head, 37 cases.

These cases must be considered as calamities, the disasters of surgery which have taken place in my Hospital practice during eleven years.

Twenty-six cases of persons in whom the skin was unbroken, and in whom antiseptic management failed to prevent organismal poisoning. Two cases of secondary hæmorrhage due to sepsis. One case of double hare-lips. One case of carbolic poisoning following excision of the hip-joint in a boy seven years old. One case of rupture of middle meningeal artery from fissured fracture of the skull in which trephining was not performed. One case of sarcoma of the synovial membrane of the knee-joint, which was supposed to be chronic rheumatic arthritis, and in which the operation was delayed. One case of excision of the knee-joint. One case of radical cure of inguinal hernia in a child 16 months old. Three cases associated with anæsthesia.

In looking back on the cases in which sepsis occurred, I am of opinion that the lesson to be learned by me is—more thorough purification of the skin before operating. It may also be noted that during eleven years many different antiseptics have been tried in the search for a reliable non-irritating antiseptic. The carbolic spray was used during the whole time except for one year, in which corrosive sublimate irrigation took its place.

The above record of the deaths which have taken place in the wards under my care is preliminary to a consideration of all the cases. I am now proceeding with a tabulation of those cases, and I hope that they may at a future time be laid before the Society.

Dr William Taylor said Professor Chiene had started several questions which were well worthy of consideration. There was the question how the two cases of inflammation of the lungs got the inflammation,—whether it was due to the severity of the injuries or to over-ventilation, which in hospitals might be inimical to the recovery of persons with weak chests. Patients sometimes complained very much of draughts, and it was admitted that phthisical

patients did not get on in the general wards, and any one with latent chest weakness would not benefit by excessive ventilation. The excess of antiseptics was another question of importance. One or two cases of deaths in private practice must be attributed to some such cause—the chilling effect of the cold spray and absorption of carbolic in wounds with large surfaces.

Dr Allan Jamieson, in speaking of the cases in which pneumonia occurred after burns, said that where there was extensive congestion of the skin from exudation or psoriasis or exfoliative dermatitis, the patients were peculiarly sensitive to cold. The regulative function of the skin seemed to be disturbed, and there was a shrinking from a cold atmosphere which was quite remarkable. The blood was retained in the skin to an abnormal extent, and the evaporation from the surface could not be lessened or increased as under normal conditions. The action of slight cold in such sensitive conditions acting on a small surface of body might cause congestion of internal organs. In the City Hospital they had noticed that erysipelas and inflammation of the lungs seemed sometimes to run hand in hand. Cases of the latter were sometimes sent in by mistake as cases of continued fever, and frequently at the same time an increase in the number of erysipelas cases was noticed.

Dr Ireland, in remarking on Mr Chiene's classification of the causes of death, said he did not think there was ever one cause for any single death. Probably some of the patients died from a combination of all the five causes.

Mr F. M. Caird thought a record of Mr Chiene's own experience would have been more instructive than one in which he dealt with the cases of the assistant-surgeons as well as his own. As to the cases of death during anæsthesia, he did not think that Mr Chiene had been present at any one of them. In the case of carbolic acid poisoning it would have been instructive to have had the signs and symptoms given on which the diagnosis was founded. He recollected Dr Angus Macdonald saying that the deaths which in his earlier practice he ascribed to the antiseptic he would in his later years put down to septicæmia.

Mr Miller remarked on Professor Chiene having given up carbolized catgut because of its being antiseptically unreliable. He thought that for this the tradesman was more to blame than the surgeon. Catgut required a long time to be properly prepared, and when there was a great demand for it they were sometimes supplied with what was literally shoddy catgut. The first case that drew his attention to this was one of excision of the mamma, in which suppuration took place at the points of sutures only. The same thing occurred in cases which followed, and did not cease till the catgut was given up.

Dr Cotterill said, with regard to septic pneumonia in connexion with burns and putrid wounds, he was much struck with what he saw a few weeks ago in a small ward in the Infirmary. There were

in it three cases of removal of the jaw and tongue. One of these took erysipelas, and two days after the other two had septic pneumonia. The ward was thoroughly disinfected. He (Mr C.) then came on duty. One case took septic pneumonia after excision of the tongue. Another did the same after removal of an axillary tumour, and he learned subsequently that two friends of the man who had undergone excision of the tongue took pneumonia after visiting him. As to the death after operation for harelip, he had a case in which death occurred from suffocation. The child died during the night when left by the mother for a few minutes. On looking up the subject there were other cases on record of the same thing. The purification of the skin, to which Mr Chiene attached so much importance, was a matter of great difficulty, and its impurities were a source of failure and danger. He did not know what to do to remedy this. Mr Edington, he observed, despaired of cleaning instruments except by boiling them for an hour or soaking them in carbolic lotion for twenty-four hours. This they could hardly do with their hands.

Mr Miller considered that there was a difference between hands and instruments, inasmuch as the skin absorbed antiseptic solutions and instruments did not. He recommended a combination of carbolic acid and glycerine as a useful disinfectant.

Dr Cotterill said the question was not whether the skin absorbed the antiseptic, but whether the antiseptic killed the germs.

Dr Allan Jamieson observed that the skin was covered by a fine horny layer, and the great point was to keep this horny layer very smooth. Both carbolic acid and corrosive sublimate lotions broke up this layer. An overfatted soap, such as that recommended by Unna, kept the horny layer smooth better than anything else. The original soap made by Douglas of Bremen was still the best, and the smoother the layer the less likely were germs to find spots where they could lie.

Dr James Ritchie, in the course of a few remarks, referred to the difficulties surgeons had to contend with in hospital, from the fact that many of the students who assisted at operations were at the same time working in the dissecting-room. He remembered the very perfunctory manner in which they washed their hands before going to the Infirmary in those days. He also asked if the case which exhibited acute bronchitis after the administration of ether had been exposed in any way after the operation.

Prof. Chiene, in replying, said he hoped to continue these experiences, and give the Society the other side. The patient who succumbed to double bronchitis was not specially exposed to cold. He could not agree with Mr Caird. The experiences he desired to give were those of an hospital ward. He did not think the hare-lip case died from the cause suggested by Mr Cotterill.

2. TWO SUGGESTIONS FOR IMPROVING THE OPERATION OF EXCISION OF THE KNEE-JOINT FOR STRUMOUS DISEASE.

By A. G. Miller, M.D., F.R.C.S. Ed., Surgeon to the Edinburgh Royal Infirmary; Lecturer on Clinical Surgery, Edinburgh School of Medicine.

More than twenty years ago, when I was house-surgeon to the late Professor James Spence, I learned that to get a satisfactory result in strumous joints it is necessary to remove the whole of the diseased synovial membrane. To carry out this principle, Professor Spence was in the habit, when operating on the elbow, of cutting away as much of the synovial membrane as he could get with scissors. In operating on the knee he adopted a more thorough procedure, which many members of this Society may remember. This was, after reflecting upwards a semilunar flap of skin, to make a circular sweep with his knife round, and at some distance from the patella, and thus remove in one mass that bone and a considerable portion of the thickened and diseased synovial membrane. This procedure strongly recommended itself to me, and I think that I have even improved on it. My method is as follows:—After reflecting a semilunar flap of skin upwards, well above the patella, I cut through the tendon of the extensor of the thigh a little above the patella, and also through the fibres of the vasti, internal and external. In this way the synovial membrane is exposed, and it is then quite easy, cutting in the cellular tissue covering it, to push up the muscular substance, and to draw down the thickened synovial membrane, which comes readily off the periosteum, and has then to be cut at its attachment round the articular surface of the femur. In this way four-fifths or thereabouts of the synovial membrane is removed in one mass with the patella imbedded in it. Those portions of the membrane that cover the ligaments are then removed by the use of the Lister sharp spoon. The ligaments should be scraped till they appear clean and white. They are then cut through to permit of complete flexion of the joint, and the operation is completed by the removal of a sufficient amount of bone.

The above method of removing the whole of the synovial membrane may be somewhat tedious, but if systematically carried out is very thorough. The procedure is not difficult, but requires care. I have employed it for several years, and in the majority of the eighteen cases given in the following table. Of the minor details of the operation I will mention only two. The ligaments should be scraped clean before they are cut, for obviously the scraping will be easier when they are firm than when they are hanging loose. The other point is that the tendon of the extensor requires to be very carefully cut across, as it lies deeply imbedded in the diseased synovial membrane, and the latter is more easily removed if

uninjured. Extensor tendon may be sutured to ligamentum patellæ.

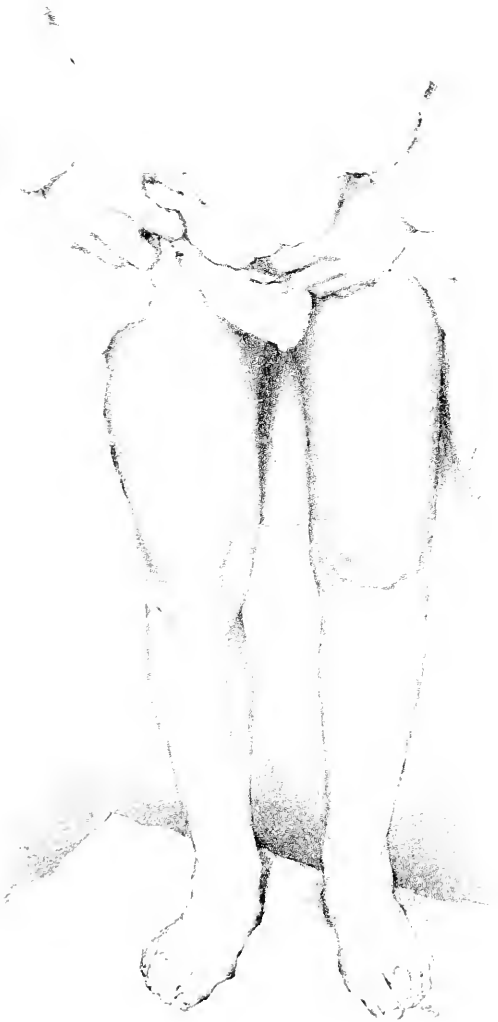
There is another procedure which I would recommend. In the old days, when a house-surgeon, I was much troubled, in dressing knee-excision cases, with the skin which lay in a loose, redundant, and troublesome manner in folds over the approximated bones. To remove this redundant skin I act as follows:—After making an incision from behind the one condyle to behind the other, across the front of the knee and at the level of the top of the tibia, I make another cut, commencing and terminating at the same points, but over the centre of the patella. These two incisions include an elliptical portion of skin, which is left attached, and the skin is dissected up over the synovial membrane from the upper cut as described above.

For this minor procedure I claim two advantages—1. Redundant and unnecessary skin is removed, which makes the limb look neater afterwards. 2. The flap of skin is more easily dissected off the synovial membrane since it is about an inch shorter than the ordinary flap made by Watson, whose method of treatment I have adopted with the above modifications.

TABLE OF ALL THE CASES OPERATED ON BY A. G. MILLER.

Name and Age.	Date of Operation.	Date of Discharge.	
1. D. M., m., æt. 23.	Jan. 19, 1885.	May 1, 1885.	102 days.
2. J. B., f., ,, 25.	Mar. 19, 1886.	May 8, 1886.	50 ,,
3. J. A. S., f., ,, 29.	June 7, ,,	Nov. 18, ,,	164 ,,
4. J. S., m., ,, 17.	June 25, ,,	Aug. 20, ,,	56 ,,
5. J. M., f., ,, 18.	Aug. 12, ,,	Amputation, Sept. 23; discharged cured, no date.	
6. K. B., f., ,, 18.	Aug. 16, ,,	Nov. 17, 1886.	93 ,,
7. J. B., m., ,, 23.	Aug. 27, ,,	Nov. 22, ,,	87 ,,
8. M. D., f., ,, 13.	Mar. 8, 1887.	May 18, 1887.	71 ,,
9. W. C., m., ,, 20.	Sept. 6, ,,	Amputation, Nov. 10; discharged cured, Dec. 8, 1887.	
10. G. B., m., ,, 30.	Sept. 28, ,,	Nov. 30, 1887.	63 ,,
11. M. F., f., ,, 22.	Oct. 27, ,,	Jan. 5, 1888.	69 ,,
12. J. M., m., ,, 42.	Feb. 21, 1888.	Jan. 17, 1889.	330 ,,
13. J. C., f., ,, 11.	April 20, ,,	May 7, 1888.	17 ,,
14. J. M., f., ,, 12.	June 22, ,,	July 28, ,,	36 ,,
15. D. S., m., ,, 7.	June 26, ,,	Sept. 1, ,,	67 ,, Arthrectomy.
16. Mrs C., f., ,, 30.	Dec. 11, ,,	Feb. 9, 1889.	60 days.
17. J. L., m., ,, 19.	Feb. 19, 1889,	Amputated May 21, 1889.	
18. W. B., m., ,, 19.	Feb. 26, ,,	April 4, 1889.	37 days.

Average duration of 15 cases in Hospital about 87 days = 12½ weeks.



Of these 18 cases, 15 were successful, 3 suffered amputation after an interval, none died. In 16 cases the excision was performed for strumous disease, in 1 for rheumatic ostitis, and in 1 for dislocation of four years' standing. In the three unsuccessful cases, amputation was recommended at the first.

In case 12, J. M., a second operation (gouging) was required for return of caries in the tibia.

The Plates represent the result in cases 14 and 18, from photographs taken by Mr J. L. Lorimer, medical student.

Prof. Chiene thought Mr Miller's idea and plan of systematic removal of the synovial membrane thoroughly good and practical, and would give it a fair trial. He did not attach so much importance to the removal of the piece of skin to prevent redundancy.

Meeting XI.—June 5, 1889.

Dr John Smith, *President, in the Chair.*

I. Election of Member.

The following gentleman was elected an Ordinary Member of the Society:—D. C. Benjamin Bell, L.R.C.P. and S.E., Kirkwall.

II. Exhibition of Patients.

1. *Professor Grainger Stewart* exhibited a case of SPASMODIC PARAPLEGIA which had existed from childhood. The patient was a young man of 23. The main facts of his family history are:—He is the second child of a family of six; there was no special feature about the labour; his father and mother and the other five children of the marriage are perfectly healthy; there is no indication anywhere of neurotic tendency in the family. In regard to the patient, his mother noticed that when a child in arms he never sat up, was always dull and listless, and inclined to be peevish. When he began to walk it was observed that his limbs "were not right," and the doctor who then saw him advised that he should be sent to the country. The present condition:—The sensory functions are normal. The motor functions show marked peculiarity. Organic reflexes normal. The skin reflexes are exaggerated. On tickling the soles of the feet the muscles of the calf sharply contract. The patellar reflex is greatly exaggerated, and the movements are begun and ended in a very abrupt manner. Ankle clonus is absent. The elbow reflex is exaggerated. Co-ordination is very defective on the left side, and this deficiency is much aggravated when the patient closes his eyes. If the left arm be extended the

fingers perform a series of rhythmical movements, and these are intensified if the patient direct attention to them. In the right arm co-ordination is practically normal. In the left leg there is a certain want of accuracy in performing definite movements, but ultimately all ordinary movements can be done. In the right leg the movements are better done than by the left, but the termination of the movement is usually brought about with a jerk. There are rhythmical movements of the toes of both feet, always worse if attention is drawn to them. The gait is spastic; the legs seem to be dragged. The moment the heels touch the ground there is a sharp contraction of the calf muscles. There is great difficulty in articulation; stammering, too, is present in very bad form. Both of these conditions are much aggravated by excitement. If the patient gets thoroughly interested in a subject and is not excited, he scarcely stammers at all. All the rhythmic movements cease during sleep. There have never been convulsions. In describing similar cases Dr Ross remarks that children are liable to be affected with a spasmodic paraplegia from disease of the vertebral column, or from any other variety of transverse myelitis. But besides these there are congenital cases occasionally met with, in which the gait is spastic, the muscles of the leg exhibit spastic features, the patellar tendon reflexes are exaggerated, and the feet generally maintained in a position of talipes equinus. The arms are not generally involved, but there is often difficulty of articulation; and the form of the head is often in one way or other altered. When the disease is confined to the legs the intelligence is often very good, but it may be diminished. There is no tendency to epileptic convulsions. Dr Ross gives four cases illustrative of the condition, two of them occurring in boys of eight, and two in boys of four years old, and in all of them the symptoms had been observed in infancy or in early childhood. In two, labour had been difficult. In two, convulsions occurred during infancy, and in the others there had been no such symptoms. Professor Stewart, after demonstrating his case, remarked that the process must be due either to a primary or secondary sclerosis of the antero-lateral columns, but he had not had any opportunity of investigating such a case after death.

Professor Grainger Stewart also introduced a patient suffering from a peculiar spasmodic affection, which may for convenience be designated as a variety of CHOREA MAJOR. Henry Hudson, a tinsmith, aet. 47, from Stockton-on-Tees. He states that his mother, who died at the age of 56, suffered from a disease like his for the last three years of her life; and a sister, who died at the age of 40, suffered from it for the last four years of her life. In her case the disease first appeared during her third pregnancy. He had six brothers, and other four sisters, none of whom exhibited any nervous disease. He was till five years ago very intemperate, but since his illness began he has been a teetotaler. His home is healthy.

His work exposes him to great heat, but no poisoning occurred among his fellow-workmen. Before his illness he fell from a roof; he first landed on his head, which was cut; and he was unconscious for a time, but soon recovered. Two months later a similar fall occurred, but he lighted on his hand, and was not unconscious. To these falls he attributes the onset of his symptoms. The first symptom occurred five years ago, and consisted in slight shaking or jerking of the muscles of the leg on voluntary movement; at first slight, but gradually increased till he experienced difficulty in walking. The jerking could not be controlled by the will. He gradually got worse. The arms began to be affected two years after, and only on making effort. About six months ago jerkings in the arms became constant, and the head and face showed slight movements, and he became unsteady when his eyes were cloudy or when he was in the dark. Four months ago the movements began to persist during sleep. Speech has been affected for two years. The pupils react to light and accommodation. There is no nystagmus. There is slight deafness. He has had much mental worry and anxiety of late. He is healthy in all respects except for the nervous symptoms, and a localized eczema on the outer surfaces of both forearms. The nervous system shows no sensory change. The organic reflexes are natural. The plantar and other skin reflexes are well marked. There is no ankle clonus. The patellar tendon reflexes are well marked on both sides. There is no paralysis of any muscles, but the voluntary movements are interfered with by the jerking movements. Co-ordination is only disturbed by these involuntary movements, and the main directions of any movement attempted is attained. When he lies at rest in bed there is irregular and coarse choreic jerking, which now ceases during sleep, but he says at one time this persisted. The right arm is more affected than the left. He generally lies with his hands grasping the bar at the top of the bed. The movements are precipitated if he strokes his head. The other nervous functions are practically normal, but there is slight excess of sensitiveness to the hot sponge on the lower part of the lumbar region and over the sacrum. After pointing out the features of the case, Prof. Stewart said that he was not able at present to indicate the precise seat or nature of the lesion. The patient is taking arsenic, and having shower baths, bromides, and hyoscyamine. Other remedies having failed, it is intended to try and subdue the habit by the use of chloroform.

2. *Dr Allan Jamieson* showed a man, aged 32, suffering from a peculiar form of BALDNESS, evidently the result of a folliculitis extending from a common centre. This was the second case of the kind he had seen, the first being that of a little girl whom he had also shown to the Society. The present case, when five years old, suffered from a pustular affection of the head. Since then he

had been subjected to repeated attacks of inflammation of the individual follicles, followed by the loss of hair on the parts affected. On the back of the neck the same condition had arisen, and had gradually extended, leaving a part bare, and with a cicatricial appearance. Under the use of ointments of sulphur and ammoniated mercury the condition had become quiescent, and no more hairs had fallen out. This condition had been drawn attention to by Quincke, who had described it as a *destructive folliculitis*. The same affection was not uncommon on the cheeks, causing a form of sycosis. There was reason to believe that there was a still slighter form in which there was almost no pustulation, and which had been confused with alopecia areata. Apparently anti-parasiticide remedies did most good. The girl got well under the same ointments as the man was using. There was nothing specific about it. Quincke had described a microbe which, when cultivated and rubbed into the skin of a rat, had produced baldness of the same character.

3. *Mr A. G. Miller* showed a boy suffering from LUPUS OF THE TONGUE.

4. *Dr Scott Lang* showed a woman who had been operated on by Dr P. M'Laren for VARICOSE VEINS. A long elliptical portion of skin, measuring 2 inches at its widest part, was removed from the back of either leg. The incision extended from the back of the knee to the tendo-Achilles. A natural elastic stocking had thus been formed with a very satisfactory result.

III. EXHIBITION OF SPECIMENS.

1. *Dr Maxwell Ross* showed a LARYNX and TRACHEA from a case of DIPHTHERITIC CROUP. The patient was a girl, aged 18 months, of a very strumous habit. She was seen in consultation with Dr Macdonald Robertson, and as the stenosis was rapidly increasing, it was resolved to perform intubation of the larynx. This was done by means of O'Dwyer's instruments, and the ease with which the operation was performed and the instant relief given contrasted favourably with the difficulties surrounding the operation of tracheotomy in children of that age. She wore the tube for sixteen hours, when a violent cough occurred, and it was expelled from the mouth. It was replaced within half an hour, but the relief was not so great as on the first occasion, and death occurred in about an hour. At the post-mortem examination false membrane was found extending down to the bronchi. Though the case ended fatally, the experience gained was favourable to the operation of intubation. There was no difficulty with the swallowing of liquids. Brandy, water, and milk were taken with ease, and even greedily, by the child. Dr O'Dwyer had pointed out that many of the difficulties in swallowing were due to the condition of the epiglottis rather than the presence of the tube, and in this instance the

epiglottis was unaffected. Malposition of the tube might, however, be a cause of difficulty, the apex of the triangular head being turned forward so as to project against the epiglottis instead of resting between the arytenoid cartilages, as intended by O'Dwyer.

2. *Dr A. Bruce* exhibited in the side-room a series of PAINTED CASTS of pathological specimens in Cathcart's medium.

IV. Original Communications.

1. *Dr G. A. Gibson* gave an abstract of a paper on CHEYNE-STOKES RESPIRATION, which is being published in full in the *Edinburgh Medical Journal*. He mentioned a great variety of affections in which it had been observed, all of which are characterized by a lowering of the activity of the nervous centres, and described the different symptoms with which this phenomenon is usually associated. In regard to the probable causation of this respiratory rhythm, he discussed the most important of the theories which have been advanced to account for its occurrence, classifying the modern views according as they attributed the periodic changes of the breathing to alterations in the cells of the nerve centres produced directly by the effects of exhaustion or of some analogous condition, or to alterations in these centres produced indirectly through modifications in the state of the circulation. The latter class of explanations, such as the hypothesis of Filehne, not only failed to account for many of the appearances present, but directly opposed the facts, and were therefore untenable. The former class, and more especially the theories of Luciani and Rosenbach, explained all the appearances present, and met all objections which had been urged against them; they were therefore alone worthy of acceptance. In conclusion, he called attention to the light which had recently been thrown on the whole subject by the researches of Marckwald.

The President said he felt sure he was only giving expression to the desire of the Society when he conveyed its thanks to Dr Gibson for his very valuable communication. The subject was an important and exceedingly interesting one. So far as the automatic and the reflex nature of the respiratory act seemed to be concerned in this affection, it appeared a point where some of the physiologists he saw present might render considerable assistance. The respiratory centre comprehensively, and without reference to the possible existence of a direct as well as a reflex region, so to speak, had been alluded to; but the characteristic symptoms of the disease suggested that they might perhaps to some extent be analogous to the irregular respiratory movements occurring after division of the sensory nerves, and supposed to be due to the passage of

blood deficient in oxygen stimulating an inspiratory centre, and of blood surcharged with carbonic acid acting in the same way on an expiratory centre in the medulla, and independent of the function of the vagi. He trusted a full discussion on the paper would take place.

Professor Grainger Stewart mentioned the case of a gentleman he had known for a very long time who exhibited Cheyne-Stokes breathing regularly, and without any marked increase, for a period extending over twenty-five years. He had never met with another case that exhibited this in such prolonged and regular fashion. The patient died, at the age of 60, of fatty degeneration of the heart, with pulmonary apoplexy and a low type of pneumonia supervening. As to the theories, it seemed to him that the views of Luciani and Rosenbach were those to which most importance attached at the present day.

Dr James said his observations on Cheyne-Stokes breathing in connexion with sleep led him to believe that the waking period was towards the end of the pause. If they could get tracings of the respiration before it was well marked, they would get a good deal of information as to its ætiology. He believed the difficulties of breathing experienced by heart cases during the night were indications of the commencement of Cheyne-Stokes respiration. A pause occurred, the patient became uncomfortable as his blood became venous, and, wakening up, got relief and then went off to sleep. The time when these cases wakened up was at the end of the pause, when the blood was most venous.

Dr Clouston remarked that in the third stage of general paralysis of the insane, where they had the greatest amount of atrophy of the brain cortex, just before death they had the mode of breathing he had always called Cheyne-Stokes. It was essentially a rhythmical respiration. He was pleased to see that those who had most carefully studied this subject brought this form of respiration under the law of periodicity, which covered every one of the higher nervous functions, and showed it to be an irregularity in this periodicity. (He did not admit a distinction between rhythm and periodicity.) What this theory did not explain was why the normal periodicity of breathing should be interfered with and become an abnormal periodicity.

Dr James Ritchie said that Luciani had drawn a very definite line of distinction between reflex and automatic centres, and Dr Gibson agreed with him. Luciani believed that the latter have the power not only of transforming impulses, but of originating action. Dr Ritchie believed an automatic centre to be an arrangement of nerve cells, whereby sensory impulses, whether from the higher centres or from peripheral end-organs, may be converted into *co-ordinated* motor impulses usually of a complex kind; but that the automatic centre has no autogenetic power, it can only liberate energy in response to stimuli, although these stimuli are

not at first sight apparent, and in ordinary working are independent of the action of the higher centres. He thought that the series of morbid phenomena under discussion would be better understood if we recalled certain physiological facts. Certain elementary structures had peculiar functions,—the function of muscular fibre is to contract under certain stimuli; the function of the nerve cell is to give out a certain kind of energy when stimulated; the period of activity is in both cases followed by a period of rest. If the stimulation is repeated the discharge of nerve energy is renewed, the amount of energy being in proportion to the stimulus. But a nerve cell may be exhausted, and its power to respond to stimuli depends not only upon the nature and frequency of the stimulus, but also upon its nutrition from second to second by the blood circulating near it. In pathological states nerve cells vary as regards their irritability, and as regards the quantity and quality of nerve energy which they give off. He understood that the cells of the medulla do not differ from other nerve cells in their nutritional relations, and he believed that Filehne, Blaise, Brousse, and others, are completely in error when they assert that the cells of the medulla are stimulated to action by carbonic acid in the blood circulating round them. There is no evidence that carbonic acid does not act on these cells as on other nerve cells—lowering their vitality and diminishing their irritability. They are nourished and they respond to stimuli in the same way as other nerve cells. The special stimuli for the respiratory centre, in the relation in which we are considering it, viz., apart from voluntary and involuntary impulses from the higher centres, are impulses from the peripheral terminations of the vagi in the lungs and of the systemic nerves in the tissues. In viewing the affections in which Cheyne-Stokes respiration occurs as a symptom, one is struck with their variety; but there is one condition which seems to be common to them all, that condition is a lowered vitality of the central nervous system. The fact that Cheyne-Stokes respiration occurs during sleep, during hybernation, after exhaustion, during the action of certain drugs, goes to prove that a lowered nerve tone is required for the production of the phenomenon. Dr Ritchie had under his care for some years two patients in whom the variability of the symptoms was remarkable. In one of these, an old lady, the symptom was not very evident when her health was fairly good; but whenever her general tone was lowered, or if the blood became loaded with morbid material during attacks of gastro-intestinal disorder, the symptoms became very pronounced —so much so at night as to interfere with her daughter's sleep. He believed that Cheyne-Stokes breathing was only one of several symptoms which arose in connexion with a common cause, but which are not necessarily dependent upon each other. One or more of them may be absent—the periodic alterations in the pupils, of the pulse, muscular twitchings, changes in the mental

state. He agreed with Rosenbach that these might be grouped under the name of the Cheyne-Stokes phenomenon. The conditions essential to their production are—a lowered tone of the nerve centres, an altered irritability, and an increased tendency to exhaustion of the nerve cells. The sequence of events in Cheyne-Stokes respiration would be thus explained (somewhat in accordance with Traube)—after a period of excessive respiratory action the peripheral stimuli diminish, the nerve cells in the respiratory centre are exhausted, and there is a period of apnœa. During this time the nerve cells are recovering energy, and at the same time waste products are accumulating round the extremities of the nerves afferent to this centre, most rapidly round those of the vagus. A gradually increasing stimulus is sent on to the centre, and it responds mildly; it is proved experimentally that no amount of stimulation by the vagus can produce dyspnœa; but the stimuli from the extremities of the systemic nerves now come into play, the combined impulse now reaches the recuperated nerve cells; these being in a debilitated condition act in an unusual fashion, and a greater amount of energy is sent out. As the blood becomes ærated, the peripheral stimuli firstly from the systemic nerves then from the vagi diminish, they act upon exhausted cells producing a diminishing result, a period of apnœa succeeds, to be again followed by a similar cycle of events. The alternation from the rhythmic to the periodic type occurs in consequence of an altered condition of those nerve cells, the activity of which dominates the regularity of the function. When the heart of the frog is removed from the body the same alteration occurs, with an additional factor. The cardiac ganglia have their vitality lowered, and the peripheral stimuli from the walls of the heart to these ganglia are altered in consequence of the absence of the circulating fluid.

Dr Gibson said he had had several cases in which Cheyne-Stokes respiration had lasted over several years, and one case related to him by Dr Muirhead had gone on for over twenty. As to the waking of a patient exhibiting Cheyne-Stokes respiration, it might occur at any period, but periodic awaking almost invariably occurred at the height of the forced breathing. Dr Clouston's remarks went to the root of the matter, and he confessed that the explanations did not get at the real reason of the phenomena. He thought he had seen periodicity of consciousness occurring in cerebral meningitis without any Cheyne-Stokes respiration, and, if he were correct in this observation, the fact went a long way in proof of the views of Luciani and Rosenbach. He was, finally, inclined to agree with Dr Ritchie's ideas on automatism and reflex action.

2. THE SURGICAL TREATMENT OF EMPYEMA.

By Albert E. Morison, M.B., C.M., M.R.C.S. Eng., Hartlepool.

The following cases of empyema have occurred during the last six years in my brother's and my practice, and I am indebted to him for notes of many of the cases, and for kind advice and assistance in difficult cases.

Case I.—A. C., æt. 6½ years, had scarlet fever ten months ago, from which he never satisfactorily convalesced. He has steadily lost flesh, increased in weakness, and been troubled with a severe cough, profuse purulent expectoration, night sweats, occasional vomiting and diarrhœa, and loss of appetite.

When brought to me on 12th January 1882, he was extremely emaciated, and appeared to be in the last stage of consumption, which opinion had been given by his regular medical attendant.

Physical Examination.—I found the usual signs of fluid at the base of the right lung, but no sign of disease elsewhere. From the amount of pure pus (about ℥x.) expectorated every day, together with the physical signs, it was evident that a fistulous opening existed in the lung.

On 14th January the chest-wall was incised, and 7 oz. of pus evacuated. He gradually lost all his old symptoms, and in seven weeks was quite well and the chest healed.

Case II.—J. W., æt. 8 years. His illness commenced suddenly six weeks ago, with what his medical attendant said was pneumonia. From this attack he gradually recovered, but the cough remained. He continued to get thinner, had difficulty in breathing, severe fits of coughing, and sweated profusely, especially at nights.

On examining the chest I found signs of fluid at the right base, and on 12th February 1882 removed by aspiration 12 oz. of pus. At first he was entirely relieved, but by the fourth day his difficulty in breathing had in great part returned, and fluid was reaccumulating in the chest. After this he gradually recovered, and by the middle of April was quite well. Syr. ferri iodid. in ℥j. doses t. d. was given.

Case III.—C. S., æt. 5 years, said to have had bronchitis for seven weeks, which is getting worse. His chief symptoms were shortness of breath, amounting to orthopnœa, severe spasmodic fits of coughing, emaciation, and sweating. Signs of fluid in the left side were discovered, the heart being displaced considerably to the right.

On 16th June 1882 I aspirated his chest and obtained 17 oz. of pus. He was at first considerably relieved, but on 21st June, all his symptoms having increased and the difficulty in breathing

being urgent, I incised the chest and 1 pint of pus escaped. His condition while under chloroform was unsatisfactory till the incision was made, but as the pus escaped it improved. By 22nd July he was cured.

Case IV.—G. B., æt. 43. Has had a cavity at his left apex for some years; seven weeks ago an attack of acute pneumonia at the left base. His present complaints are of a cough, with profuse expectoration, shortness of breath, and loss of flesh. From being a stout man he has been reduced to a very thin one. The cough, he says, comes on at intervals, and at these times a large quantity of pus is expelled by it. Relief is obtained for a time, and then the coughing and expectoration are repeated. Physical examination shows that the apical cavity is 4 inches in extent, that posteriorly and laterally the left base up to the lower angle of the scapula is dull on percussion, that the vocal fremitus is unaltered, and that crepitation and increased vocal resonance can be heard over the dull area. The aspirator needle, introduced on 23rd June 1882 between the sixth and seventh ribs posteriorly, proved the presence of pus. The chest was incised at this spot, and 9 oz. of pus escaped. The immediate result was a great improvement in all his symptoms. The tube was removed in three months, and the sinus closed, but crepitations continued over the whole of the left base.

Case V.—J. M., æt. 2 years. During convalescence from scarlet fever was affected with cough and breathlessness, which are increasing. Signs of fluid at left base. Aspiration 14th July 1882, 6 oz. of pus removed, giving great relief. Incised, owing to reaccumulation, 24th July; 8 oz. of pus. Cured 6th September.

Case VI.—O. L., æt. 6 years. Chest troubles commenced after an attack of typhoid fever, and have gradually got worse. The difficulty in breathing is so great he cannot lie down. Profuse sweating, slight cough, and frequent vomiting are the other symptoms. Signs of a large collection of fluid in the left chest, displacing the heart to the right of the sternum. Incised 8th April 1883; 1 pint of pus. Cured 30th April.

Case VII.—C. O., æt. 8 months. Has been attended for two months with pleurisy and effusion on the left side. The effusion, which at one time occupied the whole left chest, has steadily decreased, but latterly the baby has been rapidly losing ground. Cough is spasmodic and severe. Night sweats, vomiting, and emaciation decided me on opening her chest. This was done on 23rd May 1883. The pus was so thick that only 1 oz. escaped at the time of operation, but during the night a large quantity came into the dressings. The child appeared in no way benefited,

and continued steadily to lose ground until 23rd July, when it died in a convulsion.

Case VIII.—M. L., æt. 23 years, came complaining of shortness of breath on 4th February 1883. The usual physical signs of fluid in the left chest were present. Paracentesis was performed, but the fluid reaccumulated in a short time after every tapping, and so on 4th March I incised the chest. The fluid was sero-purulent. Cured 7th April.

Case IX.—C. W., æt. 4 years. First seen on 24th July 1883; complains of shortness of breath, weakness, and loss of appetite. Physical examination shows the presence of fluid in the left pleura. The symptoms became worse, and he had high temperature and night sweats. On 30th September a large aspirator needle was introduced into the left pleura, the parents of the child not having allowed anything to be done previously; nothing came. Feeling confident that pus was present, on 1st October I incised the chest and evacuated 4 oz. of very thick inspissated pus. The case went on well, and was healed on 5th November.

Case X.—E. G., æt. 4 years. First seen on 28th January 1884, with the usual signs of pleuritic effusion on the left side. Pus diagnosed on 5th March. Chest opened and drained 6th March, 2 oz. of pus escaping. Cured 20th April.

Case XI.—J. M., æt. 30 years. First seen on 12th April 1884, when the hypodermic needle introduced into the right pleura drew off pus. Operation 15th April. Cured 20th May.

Case XII.—T. B., æt. 9 years. First seen 12th March 1885. Physical examination revealed pleurisy with effusion on the left side, there being absolute dulness all over, and the heart displaced to the right side. Aspirated 20 oz. of clear serum. On 28th March again aspirated, and withdrew 17 oz. of flocculent serum. On 7th May repeated the aspiration, and got 14 oz. of pus. Chest incised on 18th; 1 pint of pus. Unfortunately I allowed it to become septic; and, in spite of a variety of measures, it would not close. He went on in this condition for eighteen months, gradually getting weaker; but his parents would not consent to any further operation till 27th January 1886, when I excised portions of the fifth, sixth, and seventh ribs. The incision was made slightly posterior to the mid-axillary line. A counter-opening was also made posteriorly in the sixth interspace, and a tube passed from one opening to the other. In shortening the tube it was done by pulling it through the posterior opening, so that the lateral incision could heal. Healed 26th March.

Case XIII.—W. E., æt. $3\frac{1}{2}$ years. First seen 25th November

1886. Complains of severe cough and copious, sour-smelling expectoration. Pus in the left pleura communicating with bronchus by a fistulous opening. Incised 9th December; healed 30th.

Case XIV.—J. S., æt. 22. First seen 14th November 1887. Complains of breathlessness, night sweats, and loss of flesh for some months.

Physical Signs.—Diminished vocal fremitus on right side posteriorly. Dulness all over back of right side, absence of breath-sounds and vocal resonance, and the ordinary signs of a large vomica at the right apex. There is a bronchial fistula, through which about 10 oz. of pus are daily expectorated.

Nov. 15.—Chest incised; 1 quart of pus. After this he improved in every way. On 12th December commencing phthisis at the left apex was discovered; 29th, complains of breathlessness and fainting, due to cardiac failure.

Jan. 1, 1888.—Died suddenly.

Case XV.—C. H., æt. 4 years. First seen by me 8th August 1888. The history was that two and a half months ago he began to complain of weakness, loss of appetite, and shortness of breath. A medical man saw him, and said he had right pleurisy with effusion. He grew worse, and pus was diagnosed, and the chest opened in the eighth interspace in the anterior axillary line. No spray was used, and the dressing of the wound appears to have been neglected. He grew worse, and after two months the mother consulted me. At this time the sinus was discharging about 8 oz. of pus daily, and the boy was extremely emaciated. With her consent, on the 10th of August I excised portions of the seventh and eighth ribs. The boy rapidly improved, and the wound closed in three weeks.

Case XVI.—J. S., æt. 2½ years. First seen 13th September 1888. The usual signs of empyema on the left side, which the hypodermic needle confirmed. The child had been ill for a fortnight, and was extremely thin. Opened chest 14th September; 1 pint of pus. Cured 6th October.

Case XVII.—W. P., æt. 9 years; has been ill since the second week in May, suffering from right pleurisy with effusion. First seen by me on 12th July, and empyema diagnosed. The father refused operation till 17th October. The lung was quite collapsed, and the boy in a very grave condition. About 16 oz. of pus escaped. Cured 11th November 1888.

Besides these cases, there have also come under my notice three others in which the disease was allowed to run its course. They all occurred in children under 10 years of age, and in each case the parents would not consent to operation.

Case XVIII.—After a severe illness, followed by œdema of the chest-wall, the pus escaped externally. During the year of convalescence that followed four sinuses appeared, each before opening causing pain and constitutional disturbance. Finally the child recovered with a collapsed lung.

Case XIX.—In this case the day before paracentesis was to be performed a violent fit of coughing resulted in the expectoration of half a pint of pus, and after a fortnight's gradually diminishing expectoration recovery followed.

Case XX.—When first seen, two months previously, the left side of the chest was nearly full of fluid, which the hypodermic needle proved to be pus. The parents refused operation, and the child, after a slow and tedious convalescence, recovered, the pus being gradually absorbed.

Remarks.

From the above cases, which have all occurred within the last six years, it would appear that empyema is more common, especially in children, than is generally supposed. It may be overlooked owing to there being only a thin layer of fluid or to too hard percussion. The breath sounds can, as a rule, be heard distinctly on the affected side, but are commonly tubular in character. The vocal resonance is in some cases not diminished, and in some is increased.

These cases also show that the disease is not serious, and very amenable to treatment. Of the twenty cases reported only one terminated fatally, and even this cannot be said to be due to the treatment (Case VII.) The patient was a miserable starved infant of 8 months, and at the time of death, which took place in a convulsion, the chest showed considerable improvement. In five cases prior to operation pus had been expectorated through a fistulous opening communicating with a bronchus. In one case (XIX.) a cure without operation was effected. Although this happened, these cases pursued a thoroughly septic course; after the operation the fistula closed and the lung expanded freely.

That pus in the pleural cavity is capable of absorption is proved by Case XX., where operative interference was not allowed, and the pus disappeared, and by Case IX. In this the parents refused operation for six weeks, and on introducing a large aspirator needle no pus escaped. When the chest was incised, 4 oz. of thick inspissated pus came away, and I have no doubt, had a longer time been allowed, it would, as in Case XX., have been entirely absorbed.

In two cases (Nos. IV. and XIV.) the primary phthisis of the left and right lungs respectively was complicated with empyema. In both of these, too, there was a bronchial fistula communicating

with the pleural cavity, through which pus in large quantities was daily expectorated. As in the other cases of fistula, on draining the pleura the purulent expectoration ceased, and the lung fistula closed. Both these cases went on satisfactorily as far as the empyema was concerned—one ending in ultimate recovery, the other in death from cardiac failure on the forty-fifth day after the operation, but for some days prior to his decease the discharge had been serous, and only enough to stain the dressings.

In two cases (Nos. XII. and XV.) I had to resort to excision of portions of ribs.

Diagnosis.—A short, dry cough in a child with ashy complexion, who sweats freely, and more especially at night and about the head, with signs of pleuritic effusion and febrile temperature, is almost pathognomonic of empyema. The diagnosis can only be made absolutely certain by the use of the hypodermic needle. I believe this may always be used with safety if the skin round the puncture be purified and the needle made aseptic. In some cases two or three punctures are necessary before the result is absolutely certain.

Operation.—The patient is prepared in the usual way as for any operation. The side of the chest is previously washed with soap and warm water, and carbolic lotion (1–20). Chloroform is administered with the patient lying on his back. The side to be incised is made to project over the edge of the table to avoid turning the patient on to his sound side. In all cases the spray is used both at the operation and during the after-treatment. This I look upon as one of the most important details necessary for success. The only two cases in which I have been called upon to perform Estlander's operation were those in which the spray was not used.

Situation of Wound.—The method I have adopted in all my cases is to make, first, a vertical incision through the skin in the posterior axillary line, then a horizontal incision through the muscles into the pleural cavity along the upper border of the seventh rib. The handle of the knife is then introduced into the wound, and a pair of sinus forceps is slid along it into the pleura, and the wound dilated. A large-sized drainage-tube about 3 inches long is inserted. The tube is perforated for the inner third of its length. If there are any holes in the outer half, granulations grow into them and block the tube. A safety-pin is passed through the end of the tube to prevent it slipping in, and it has also this advantage over tubes that have an expanded end to apply to the chest-wall, that the tube may be shortened without pulling it entirely out of the pleural cavity. As day by day the tube is shortened the pin is removed, the tube pulled out about half an inch, the pin reinserted, and the projecting end of the tube clipped off. I have never required to excise a portion of rib to allow of free drainage.

The dressings now used are a layer of carbolic gauze, over this

a wood-wool pad, and a bandage firmly and carefully applied. If necessary, the wound is dressed the same evening, and it is generally advisable to do so. The dressings are changed every day for a week, and after that time every second day is generally sufficient. The tube is shortened in about four days, and thereafter every third day, according to the amount of discharge. The tube requires changing for an unperforated one when about an inch and a half have been removed.

During the third week the dressing is less frequent, and in a number of cases the tube can be removed, and the wound rapidly closes.

The discharge, when all the pus has been evacuated, after incision is normally serous. Should it continue purulent after the third or fourth day, it shows either that the tube is not acting efficiently or that the wound is septic.

In all the cases which have been treated by simple antiseptic incision and drainage the lung has expanded so completely as to leave no retraction of the chest-wall, even where there has been abundance of pus, or the case has been left untreated for a sufficient time to allow of absorption of a great part of the fluid. In Case XVII. the lung was absolutely compressed for six months before operation, so much that there was considerable lateral curvature of the spine. Two months after the closing of the wound the right half of the chest measured the same in circumference as the left, and all traces of curvature had disappeared.

This result I attribute to the absence of irritation from putrefaction or antiseptic irrigation, which it appears to me leads to chronic inflammation with thickening and subsequent contraction and cicatrization of the thickened pleura. The result in the two cases may be compared to the pliability of a cicatrix, the result of primary union, and of that by granulations. The former is soft, pliable, and non-adherent; the latter firm, non-distensible, and adherent to the underlying bone. In empyema the lung should expand if the irritation by injections and putrefaction is avoided, because I think these cases show that aseptic pus does not lead to so marked thickening and contraction of the pleura as to cause compression and binding down of the lung by adhesions.

As an aid to assisting the lung to expand, immediately the patient is able to be out of bed he should be encouraged in the use of gymnastic appliances, *e.g.*, dumb-bells, hanging and swinging on trapeze, etc., and in making powerful expiratory efforts, such as running up and down stairs, playing wind instruments, etc.

To sum up, my conclusions are:—

1. That in the diagnosis of empyema the only certain sign is obtained by the exploring needle, and that this, carefully used, is perfectly safe.

2. That antiseptic incision, the wound being just sufficiently large to admit a full-sized drainage-tube in the posterior axillary

line, and made with the patient on his back, drainage and careful dressing are all that are required.

3. That the average time for healing, in uncomplicated cases, should not exceed four weeks.

4. Double openings, irrigation of the pleura, excision of the ribs, and such other devices, are unnecessary and harmful if the case be seen before an opening has formed through the skin.

Prof. Chiene congratulated Dr Morison on his results, and said he was glad to see him still hold to the earlier views with regard to the use of the spray in antiseptic surgery. He agreed with him that excisions of portions of several ribs was not required except in cases allowed to become septic; but in his experience of empyema in adults he found it sometimes difficult to get free drainage without excising subperiosteally a portion of one rib.

Dr Bedford desired to draw attention to one part in Dr Morison's paper, viz., the use of the hypodermic needle for making the exploratory puncture of the pleura. In the first of a series of cases under Dr Bedford's care at the Gesto Hospital, he had made such a puncture, *secundum artem*, with an extra long hypodermic needle, as he had been taught to do at Edinburgh. The puncture was made below the angle of the scapula posteriorly, and the case was one of purulent pleuritic effusion of some months' duration in a lad of 20. The result was the patient started, levered the needle against his more than normally approximated ribs, and broke it into the puncture. Although a cross incision was at once made over the needle, it was not secured till a few days later, when Dr Bedford opened and drained the pleural cavity. Professor Greenfield had informed Dr Bedford of the occurrence of the same accident to himself and to a friend of his, a well-known London surgeon, both of whom now used special needles. While doubtless a needle so introduced generally became encysted, and gave rise to no further trouble, yet the accident was an unpleasant one. Dr Bedford had since employed, without any danger of such an occurrence, a fine Dieulafoy needle, and found it worked very well.

Dr James drew attention to the value of repeated tapping in cases of empyema, which he had practised with much success. He asked where Dr Morison considered he was most likely to find pus, and at what point he made his free incision into the chest.

Mr Caird said empyema in children was very amenable to treatment, but the time given by Dr Morison for a successful result was very short. In nine or ten cases he (Mr Caird) had treated in adults he could corroborate Dr Morison in everything except that he found it of importance to excise a portion of rib.

Dr Hodsdon pointed out that the majority of Dr Morison's cases was in children, and in them it was exceptional to meet with failure if they were properly treated. In young children it was

very difficult to get in a good-sized tube without removing a portion of rib. He had not seen the spray used in any of the cases that came under his notice, and all of them got well.

Dr E. Price asked if it was necessary to use a drainage-tube in children at all. Quite lately a case occurred in his practice in which the pus discharged through the opening made by an aspirator needle, and the child did well.

Dr Webster related a case of a man in which the tube came out on the tenth day, and could not be reinserted. The patient did well. He thought the case showed that drainage-tubes were sometimes allowed to remain too long in the wound.

Dr Morison said he had not found it necessary to remove a portion of rib in any of his cases. When seen before there was an external opening, and putrefaction was prevented. The accident which happened to Dr Bedford with the hypodermic needle had also happened to him. The needle broke, but did not pass into the pleural cavity. To prevent this a stouter needle than that used in ordinary hypodermic syringes was required. In reply to Dr James, he thought the chest should be incised as soon as pus was found. In a number of cases he tapped frequently, but with the result that he had to incise afterwards. The difficulty of getting a full sized drainage-tube into the pleural cavity could be got over by the introduction of sinus forceps without excising a piece of rib. If left in too long the tube would certainly cause erosion of rib, but under antiseptic treatment the tube never required to be left in long enough to produce this effect. As to the spray, Dr Hodsdon and he differed, so that he would not discuss that point. He believed a drainage-tube was necessary in all cases. Ten days seemed a rather short time to keep it in.

Meeting XII.—June 19, 1889.

Dr Smith, *President, in the Chair.*

Discussion on Intestinal Obstruction.

The Chairman, in introducing the subject for discussion, said the pathological conditions leading to this affection were various and very different indeed, but in a society of this kind they might assume that these were familiarly understood among the Members. Certain symptoms were common to all those affections; others, however, supposed to distinguish the special lesions, were obscure and difficult, and on those the discussion this evening would probably turn, as aiding in the diagnosis with a view to the treatment to be adopted. He supposed this would be the line upon which the discussion would be directed, viz., the diagnosis of the peculiar

lesions which constituted those obstructions. He had great pleasure in asking Mr Miller to introduce the subject.

Mr A. G. Miller said,—Mr President and Gentlemen, I suppose that your Council has selected me to open this discussion because I suggested the subject as one worthy of consideration. I hope to be able to direct the discussion towards one point, so that a concentrated light may be thrown on it. This is all the more necessary because the subject is a very wide one, and the amount that has been written on it during the past twenty years is immense. Were I to attempt to give anything like a *résumé* of the literature of Intestinal Obstruction I would weary both myself and you. I will therefore limit myself as much as possible to the subject of *diagnosis*. But first as to definition, though I think this is hardly necessary.

By "intestinal obstruction" we understand, I suppose, that condition in which there is obstruction to the passage of fæces along the intestine, what is commonly called "stoppage of the bowels." For practical purposes, however, I think we will all agree to eliminate cases of strangulated (visible) hernia on the one hand, and malignant stricture of the rectum on the other.

I have selected the point of diagnosis because—(1) it comes first necessarily in point of time; (2) it comes first in importance, seeing treatment necessarily depends upon it; and (3) it is often —I may say, very often—difficult and doubtful. Diagnosis may, I think, be aided by dividing all cases of obstruction into three classes:—1. The very acute. 2. The less acute. 3. The chronic.

The first class are easily recognised. They have the general features of a case of recent strangulated hernia. These are—sudden pain, sickness, and collapse, the pain being referred to the umbilical region, the sickness being of the persistent character, quickly becoming fæcal, and the collapse being sudden and severe. In such a case, though there should be no visible hernia, we know at once that there is not only stoppage of the passage of fæces, but also a condition of the bowel (constriction or obstruction) which, if not speedily relieved or removed, proves quickly fatal.

The third class of cases also should be easily diagnosed. I will not therefore refer to them specially. One must remember, however, that there are instances in which obstruction arises from some very chronic cause, but in which the symptoms come on suddenly. Even in these cases, however, the previous history and condition of the patient, if gone into carefully, should make the nature of the obstruction clear enough.

It is in the second class of cases that there is greatest difficulty of diagnosis—those which lie between the very acute and the very chronic. It is of great importance to see them at the very first, for the initial symptoms very quickly change or become masked. A physician has a case of obstruction under his care

for some time, and then sends for a surgeon. Why? Because his treatment has failed, or because he has not been able to come to an exact diagnosis. The consultant is placed at a double disadvantage. He is expected to make up his mind at once on a case that the other has been studying for some time, and, besides, he does not see the case under the most favourable circumstances. I consider, therefore, that in every case in which obstruction exists, but in which an exact diagnosis cannot be made out, it is important for the patient that a consultation should be held at once, both a physician and a surgeon being present. The one will act as a check on the other, for each doubtless will look at the case from a somewhat different point of view, and each will thus be able to supplement the other's examination. I would also suggest that at such an examination, if doubt exists, the patient should be placed under chloroform or ether. Palpation of the abdomen would thus be made more easy and accurate.

The next difficulty is the *masking of symptoms by remedies.* These may be taken by the patients themselves, or administered by a doctor. Of these the commonest perhaps is a purgative. This does harm, not only by aggravating the existing symptoms and by weakening the patient, but by inducing symptoms that did not previously exist. On the other hand, one may doubt sometimes whether the sickness, hiccough, etc., are not mainly due to the medicines administered. Another remedy usually given is opium, or some other narcotic. These relieve pain, diminish sickness, and inhibit the tendency to collapse. To this extent they do good; but they also do harm by raising false hopes, and occasioning the postponement of the operation that otherwise might be at once determined on. Enemata are often useful, but sometimes interfere with diagnosis by causing distension of the abdomen.

Abdominal distension, it is quite plain, will make diagnosis difficult. Palpation will be most unsatisfactory, and the discovery of an intussusception, for instance, almost impossible. In cases of distension the administration of an anæsthetic may aid the examination very materially.

Peritonitis, which comes on sooner or later to a greater or less extent in every case, not only masks the general symptoms, but leads one to doubt sometimes whether the inflammatory affection is not the main or only one.

The last difficulty I wish to refer to is the *absence of some ordinary symptom.* In intestinal obstruction the one common symptom is stoppage of the fæcal passage. Every other symptom that I know of may be absent or greatly modified. Even that may be made uncertain, and therefore misleading, unless great care is taken; for the return of air injected during the administration of an enema may be mistaken for the passage of flatus, and unabsorbed meat suppositories may be mistaken for fæces. Even characteristic symptoms may be absent, and this may oblige

one to hesitate in forming a diagnosis. For instance, in a case of intussusception there may be no bloody mucus discharged. In another case vomiting may be awanting, or very slight; and this is most common, I believe, when the obstruction is below the ilio-cæcal valve. Pain may be modified by collapse, and we all know how one or even more motions may be passed after obstruction of the small intestine is complete.

The comparative frequency of these deviations from the common rule makes it necessary that we should be careful in forming a diagnosis, and should be willing and anxious in every difficult case to obtain at once the assistance of a colleague, seeing complications arise so quickly and mask the distinctive symptoms.

I believe that one of the best contributions to such a discussion as this is to give one's experience. I will try to do this as briefly as possible. I have performed abdominal section for unrelieved obstruction five times in the Royal Infirmary during the last four years.

Case I.—M. B., æt. 36, female, recommended by Dr Affleck.

History.—Sudden pain in abdomen on 3rd January 1886; no vomiting. Took two wineglassfuls of castor-oil, which caused sickness. Enemata and croton-oil administered without effect.

On admission to Medical House, 11th January, abdomen very tense and tympanitic, even over liver; colon distinctly mapped out; nothing found per rectum. Seen by Mr Miller on 13th, who advised against an operation. Dr Thomas Keith was also brought into consultation at Mr Miller's request, and he agreed that there were not sufficient grounds for an operation at that time. Three days later, patient, having become much worse, was removed to Mr Miller's wards, where abdominal section was performed. The sigmoid flexure was found crossing and bound down to the sacrum by a band of fibrous tissue, and the colon enormously distended with hard fæces. The constricting band was cut and the bowel relieved, after which three copious evacuations took place. Patient died next morning.

Post-mortem.—No evidence of recent peritonitis; ring of congestion round sigmoid; omentum adherent to intestines; three perforations in colon from ulceration, apparently caused by fæcal infarction.

Case II.—J. S., æt. 5½ months, male, recommended by Dr T. Duddingston Wilson. Admitted 3 P.M., 6th October 1886.

History.—Perfectly well till two days before admission, when he was suddenly seized with pain in the abdomen, followed by vomiting and discharge of blood and mucus from the bowels. Intussusception felt. Abdominal section recommended and performed at once. Invagination (of lower portion of ileum, cæcum, and ascending colon into transverse colon) undone, and symp-

toms relieved. Child died two days after, apparently from bronchitis.

Post-mortem.—No general peritonitis; matting of cæcum and ascending colon to adjacent intestines; lower end of ileum and cæcum congested; marked bronchitis; no pneumonia.

Case III.—F. M., æt. 52, male, admitted from waiting-room, 17th September 1887.

History.—Subject to attacks of constipation during last nine months; present attack nine days ago; came on with pain in left iliac region; no vomiting. Used enemata and purgatives; retched on one day only—the day before admission.

Condition on Admission.—Abdomen distended; colon distinctly mapped out; pain in left iliac region and over sacrum, latter very severe; constipation, but flatus is passed occasionally. *Per Rectum.*—Piles; ring round rectum high up, evidently not malignant. Patient greatly emaciated and very weak. After consultation, abdominal section was delayed, but performed on 19th merely as a *dernier ressort.* At operation volvulus of sigmoid, contraction of bowel above volvulus, and distension of colon were found. Patient died three days after with symptoms of septicæmia.

Post-mortem.—No general peritonitis; fibrous contraction at upper part of sigmoid due to old inflammation; large septic abscess almost surrounding rectum at upper part.

Case IV.—M. H., æt. 18, female, recommended by Dr Wyllie.

History.—Sudden pain in abdomen on 1st January 1889; vomiting marked and immediate; complete constipation. Admitted to Medical House 3rd January.

Condition.—Pain at umbilicus and over sacrum; no vomiting after admission; abdomen distended; no mapping of intestines; treatment by morphia, enemata, etc. Patient removed to Mr Miller's Wards 9th January, when abdominal section was at once performed. Volvulus of sigmoid discovered and undone; bowels evacuated and symptoms relieved. Death two days after, suddenly, from the shock of a return of the obstruction.

Post-Mortem.—No general peritonitis; sigmoid flexure greatly enlarged and distended with flatus, and pressing backwards on descending colon, so as to completely obstruct it; constriction of upper part of sigmoid; stricture commencing at junction of sigmoid with rectum. (See full report of case by Dr Stoddart, Resident-Surgeon, *Edinburgh Medical Journal* for May 1889, p. 1031.)

Case V.—R. S., æt. 10, male, recommended by Dr Dickie; admitted 27th February 1889. Patient was seized with severe pain in abdomen on his way to school, and immediately after had

a loose motion. This was on the 25th. He was seen by the doctor, who diagnosed intussusception, and sent him to me on the 27th for operation. The usual tumour was found in the usual situation. At the operation, which was performed at once, the invagination was readily reached; but, contrary to my expectation, could not be undone, owing to adhesions. In using considerable force the gut gave way at one or two points, its substance being very friable. Resection of the adherent portions was at once decided upon. On removing the intussusception it was found to be gangrenous and putrid. As the boy by this time was much collapsed, the ends of the bowel were brought out at the wound, and the formation of a fæcal fistula aimed at. The boy sank, and died the same night. The invagination was at the cæcum. No post-mortem was permitted.

In remarking on these cases I would like to say, first, that the fatal result, which unfortunately occurred in every instance, was largely due to complications — ulceration of colon, bronchitis, absorption from septic abscess, recurrence of obstruction, and gangrene of bowel.

In the two cases of intussusception diagnosis was easy, and the result should have been satisfactory, but was unfavourably influenced by complications.

In the other three cases it is interesting to note that they were all instances of sigmoid obstruction, in all vomiting was markedly awanting, and in two at least severe pain over the sacrum was complained of. In all of them diagnosis was made more difficult by the occurrence of symptoms, partly due to the administration of remedies, and partly to the supervention of peritonitis, for which, unfortunately, time was given.

Mr President and Gentlemen,—I know that what I have said has been very commonplace, and anything but exhaustive of the subject. I hope, however, that valuable comments and relations of experience will follow these remarks.

Dr John Duncan said he feared the subject was not one which lent itself very easily to such a discussion on account of its immense magnitude, it being the usual experience that limited subjects were more useful in that way. Even from a surgical point of view there was an immense variety of questions, an immense mass of details connected with laparotomy, enterotomy, and particularly in relation to sepsis, so that it was difficult to select matter for comment. He was inclined to agree with the Chairman and Dr Miller that the most important part of the subject was that which was connected with diagnosis,—if they could make the body diaphanous they should have little difficulty as to what treatment ought to be adopted. In this, as in most other surgical operations, the mere operation probably required

some brains, but it required extremely little manual dexterity.

Diagnosis depended so much, in every individual case, upon investigation of minute details, that it was almost impossible to lay before them, except in connexion with special cases, the points to be discussed. Perhaps it would be best that he should endeavour to indicate a classification of those difficulties rather than to deal with minute points; to ask, for instance, what were the diseases which were apt to be confounded with intestinal obstruction; and next, what were the difficulties in their way in isolating the various forms of obstruction, when they had decided that obstruction was the thing with which they had to do. He would be inclined to regard obstruction rather as divisible into the acute and the chronic than into the three classes which Mr Miller had mentioned, and to look upon the first class as those which presented, more or less severely, the three symptoms to which he had referred—the pain, the vomiting, and the collapse—because he regarded the constipation as perhaps the least valuable and most unreliable of all the symptoms they had to act upon in these cases. What little attempt he should make to classify would be rather in connexion with the acute than the chronic forms of obstruction. In the first place, what were the diseases and the conditions that were liable to be confounded with obstruction? He thought they might put it, in the first place, that they had a great class of diseases which essentially consisted in inflammation of the lining, the internal coating of the bowels, in every one of which there were difficulties in diagnosis, and mistakes might be made by competent practitioners,—for instance, dysentery, irritant poisoning, and so on. Next in the process of classification they had inflammation of the external covering of the bowel—peritonitis. Under this head, the most usual, the most common and simple forms of peritonitis present little difficulty. They had, however, various cases in which extreme difficulty was met with. For instance, they had peritonitis itself leading to obstruction very quickly, either by bands of lymph or twistings. Then they had peritonitis often arising from obstruction, very speedily in some cases. They had, again, peritonitis the result of perforation of the vermiform appendix and elsewhere, giving symptoms that were extremely like the symptoms of acute obstruction. And, lastly, they had, as resulting from peritonitis, a very peculiar class of cases, in some of which even laparotomy had been done with some success. He thought it was Mr Humphrey who had a very curious case, in which he opened the abdomen and found nothing except peritonitis, and closed it up again with absolute success. Some of those cases, he thought, undoubtedly arose from that peculiar form of paralysis of the bowel which resulted from the inflammation passing through its coats. He had a case of hernia in which he operated—rather late, he must say—

on a bowel upon which taxis had been employed. He found the bowel extremely bruised, with extreme extravasation in its coats. The patient was an old man, nearly 70, and he died within three or four days. They found, on *sectio*, simply this,—a paralytic condition of about six inches of the bowel, and above it a mass of soft fæces which should have had no difficulty in going down, and yet could not pass this, not obstructed, but paralyzed portion of the bowel. Another case, illustrating the difficulty of diagnosis, occurred to him in which a boy had tubercular peritonitis. He was supposed to be dyspeptic, but nothing more. However, one morning severe vomiting came on, becoming feculent in twenty-four hours. They diagnosed obstruction, and he opened the bowel, but found that it was simply tubercular peritonitis. The boy died shortly afterwards. These were examples of extreme difficulty in diagnosis. Lastly, there was a class of cases which they must bear in mind, which, under the third head, were apt to be confused with obstruction—that was a reflex irritation of the bowels. One very interesting case he remembered coming before him,—a case which was sent to him as a case of strangulated hernia in a boy. There was a lump, inflamed, and tender and painful, in his groin, and he had extreme collapse and vomiting. When he examined it before operating, he found that there was no testicle in the scrotum. He cut down upon this tumour, as they ought always to do in the hernial regions, and he found it to be a testicle partially descended which had become inflamed in its passage. These reflex conditions gave rise to symptoms exceedingly like obstruction. These, then, were the chief diseases—inflammation within and inflammation without the bowel, and reflex conditions—which were prone to be confounded with obstruction. When they came to the diagnosis of what was really obstruction, perhaps the first and most important class which they had to diagnose were those in which severe symptoms of obstruction supervened upon accumulations in the bowel, whether these accumulations were of foreign bodies or of fæces, or supervened upon chronic tumours, malignant or other, connected with the bowel. Out of a good many of such cases he would refer to two. One was a case which he was called to by Dr Foulis, of an old gentleman who had a very marked form of obstruction. They found a lump, which was to be felt in the left fossa, and they diagnosed the possibility of physical obstruction. He passed a long tube, which resulted in the passage of an enormous quantity of liquid fæces after it had gone through with a little pressure and with a sudden jerk. The success of the long tube here proved that in a good many cases they could pass the tube further up than in their books they were given to believe. No doubt in many cases it was utterly impossible, but the long tube was a thing which ought not to be neglected. But there was another set of cases

belonging to that class, of which the following was an illustration,—the sudden supervention of acute symptoms upon a long-standing tumour. These tumours were very often not to be felt in any way. They were frequently to be diagnosed by preceding attacks of more or less acute obstruction from which the patient had recovered. An interesting case was that of an old person, who, a year before, had an attack of obstruction, from which she recovered, and during the whole year there had not been the slightest recurrence of any trouble with the bowel. Acute obstruction again came on. He operated then. She was exceedingly fat; but even had she been exceedingly thin, he believed they should not have been able to save her. They found, upon laparotomy, that there was a small, malignant, annular stricture of the ascending colon. The bowel was collapsed below and distended above. They found it impossible to do anything, except to separate the part to bring it to the umbilicus. In the process of separating it rupture occurred, and though the toilette of the peritoneum was made, she died of septic peritonitis. In such cases, if the acute obstruction was the first attack, and no tumour was to be felt, so far as was known at present it was absolutely impossible to tell them from any other sort of obstruction. He knew no means of doing it. Lastly, they had other cases of volvulus, internal hernia, intussusception, and so on, which required to be diagnosed from each other, and which, when they were diagnosed, were perhaps surgically the most satisfactory of all the rest. He remembered a very interesting case, in which, after a diagnosis of internal hernia of some sort, he opened the abdomen and found, after some search, running, as was a very good plan, from the cæcum backwards, a tight portion of collapsed bowel, which ran down into the pelvis, and they found a portion of the bowel protruding into the structures behind the uterus. This was pulled out, and things put right. The conclusion of this classification plainly was that they should endeavour, to the best of their power, that they make their diagnosis accurate, and, from a surgical point of view, try to eliminate, in any individual case, the diseases which were apt to be confused with strangulation. In the first place, especially inflammation of the bowel and reflex forms, because in these laparotomy could do no possible good, it was absolutely necessary that they eliminate these. It was essential that they should distinguish the various forms of obstruction from each other, rather with a view to putting in practice—in intussusception, for instance—of remedies which they knew to be successful; but it was still more important that they should diagnose early the cases in which laparotomy ought to be performed. Undoubtedly the great mortality which had attended laparotomy was almost entirely due to the late period at which it took place. It was precisely the same thing with regard to hernia.

If they could operate on hernia soon before severe taxis was tried, before or immediately after inflammation had time to set in, and, above all, before gangrene, there was no operation so successful. They were almost sure the patient would do well. If they could manage to diagnose cases of internal hernia, or volvulus, or intussusception early, their success would be exceedingly great.

Dr Affleck said that as this subject was one in which he felt considerable interest from sundry experiences connected with it, he might be excused for referring to some points. The diagnosis could, in his opinion, scarcely be considered separately from the other points in connexion with it; but the subject was such a large one, that one hardly knew where to begin or how to deal with it. He entirely concurred with Mr Duncan's remarks in regard to the difficulties which arose in diagnosing obstructions from other conditions which had no connexion with it,—such, for instance, as the inflammations of the coats, mucous membrane, or peritoneum. Even here, he repeated, the difficulties were exceedingly great, and had he time he would refer to some of his experiences. He had had cases where the abdomen was opened for all the symptoms of acute obstruction, and ulceration found. Any remarks that he had to make would bear reference to some clinical aspects of the question, and they were somewhat personal and general. He meant to confine himself to cases of acute intestinal obstruction. It had been his experience as a physician to encounter cases of this kind; and while he in one or two instances succeeded in applying treatment which rescued the patient from imminent death, in a great number of instances he had been obliged to witness the swift and painful progress of the case to its fatal issue. For example, he put upon record as far back as 1873, two cases of intussusception which occurred to two children six months old (and which he watched) within nine months of each other, where treatment failed in one case, and a *post-mortem* examination showed a large part of the small intestine intussuscepted and gangrenous. The other case proved a success. In it he inflated the bowel with a pair of bellows and the child recovered. There were here all the symptoms present, such as bleeding from the bowels. The child was in a near state of collapse when the operation was done. He was, however, rescued. It was not very long ago since a case occurred in the Infirmary, where a man was brought in said to be suffering from acute peritonitis. He (Dr Affleck) went up to him and observed that his abdominal muscles were straining hard, and he came to the conclusion that it was not a case of peritonitis so much as a case of obstruction. One of the surgeons saw the case with him, and also came to the conclusion that it was a case of intestinal obstruction, and recommended opening the abdomen. The man's wife was sitting by the bedside in great distress, and she asked him

if nothing else could be done. He then resolved to try the interrupted current. The result was that the man passed a large motion and got immediate relief. He might say that in several of these instances he had invoked the aid of the surgeon, believing that his interference alone afforded a chance for life. While everything that surgical skill could accomplish in the matter of the operation was done, he regretted to say that in every case, notwithstanding that the obstruction was discovered and relieved, the result was unfavourable. In pondering over such unfortunate experience he sometimes felt inclined to blame himself for not sooner seeking and more earnestly pressing for the surgeon's aid, but he felt at the same time bound to state that sometimes the delay had been on the part of the surgeon rather than himself, and that therein lay the fact that delays often arising were particularly against the patient. He did not for a moment blame the surgeon. He had to go into the case knowing nothing previously about it, and this as well as the possibility of error in diagnosis, and the uncertainty of the locus, often tended to deter to some extent from immediate interference. Besides, there was the well-known fact that not very seldom cases which bear all the features of serious mechanical obstruction, whether truly so or not, undergo spontaneous relief in a very remarkable manner, a circumstance which tended to delay an operation, the risks attending which were so considerable. There were many instances where in cases presenting all the appearance of serious organic obstruction a sudden yielding had taken place and the patient recovered. The consequence has been that the operation has usually not been undertaken until symptoms had arisen which, to his mind, appeared to throw the balance of probable success heavily against the patient. He felt perfectly sure that when the temperature began to rise, the pulse to quicken, the face to get pinched, and the abdomen markedly tympanitic, it was nearly hopeless to expect any other than a fatal result. He was firmly convinced that success depended entirely upon early procedure. But here came in the momentous importance and grave responsibility of the physician in relation to the case. Now he was, he thought, bound to see that everything possible had been done, that every means of investigation was exhausted to exclude error in diagnosis. This naturally introduced the question of whether there was any means of attaining approximate accuracy in the diagnosis, in finding the locality of the disease. The physical examination sometimes yielded valuable positive evidence, as in the case of intussusception, but it was not very valuable as negative evidence, because there might be very serious obstruction without any evidence of it by physical examination. It was well, then, to guard themselves against supposing that any one symptom, or class of symptoms, was

capable of settling the diagnosis in a given case. This would be as dangerous a view as to hold that they had no guides or clues at all to help them. When they considered the numerous causes of obstruction, even acute, and the fact that the same lesion appears to affect different individuals differently, and that in one it might be rapid and in others the reverse, it was easy to see the difficulty. They seemed thus to be driven to what was after all the most valuable element in the diagnosis, namely, the clinical history of the case. A careful consideration of this, with, of course, the character of the present symptoms, and it might be the physical signs, would often lead one very near or even actually to the truth as regards both the nature and seat of the lesion. Probably, however, they were oftener left on the ground of conjecture. Now he desired to refer to some of the points of evidence that might be trusted. In every case he advised them to examine for hernia, and also to explore the rectum. They might find intussusception in the rectum when there was no evidence of it on the wall of the abdomen. The age of the patient might help in diagnosis. If, for instance, they were called upon to examine a child for obstruction—the symptoms being to all appearance present—they could hardly be wrong in supposing or believing that in all probability it was intussusception, the cause of the great majority of cases of obstruction in children up to the age of ten years. They would also probably find it in the commonest of all localities, the cæcum. It would be still further confirmed if with the other symptoms they found the sausage tumour, and the red discharge from the bowels, with vomiting and collapse. In addition to this, inquiry ought to be made as to the habitual state of the patient's bowels, for here they might be led to fæcal impaction, or volvulus produced by overweighted bowels. Another thing that he sometimes found of use in inquiring into this cause was the history as regards any previous inflammatory condition of the bowels, if there had been at any time peritonitis, because some cases had occurred, such as the one to which Mr Miller had referred, in which they were very apt to think of adhesions, or bands, or things of that kind, that were likely from certain circumstances to be the cause of acute obstruction. So also with regard to gall stones, etc. Now, what were the main symptoms upon which they could rely as indicating obstruction? Constipation was of less value than some others, although it certainly was valuable, and he should say that constipation together with the absence of flatus were important points in the diagnosis. If they saw the intestines inflated and no discharge downwards, then that was a point that was not to be despised in regard to the diagnosis. Then pain was a very important indication. It was important, further, to study the character of the pain. Was it a constant or an intermittent pain? Did it come on suddenly? These points were valuable as indicating

the position of matters. Vomiting was in his opinion a very important indication, and was generally said to be, and he believed it was, the more urgent the higher up the obstruction was, but that was not by any means conclusive. He did not think the vomiting necessarily depended, as some held it did, upon the greater activity of the smaller bowel as compared with the large bowel. He supposed that in many cases vomiting was a reflex condition, because it sometimes occurred very severely and constantly when the obstruction was low down. But vomiting more frequently occurred when the obstruction was in the small intestines, and the pain was more acute when the obstruction was in the small intestines. There was a means of diagnosis suggested by Mr Treves,—the injection of water and the auscultation of the colon and cæcum. He was not, however, sure that that would be useful, because obstruction might exist and allow water to go up, but not let fæces come down. The practical bearing of the question was, What relation had they to each other in the case when the physician called in the surgeon and got his help? Now they could hardly separate the treatment from the diagnosis. The usual treatment was the giving of purgatives, and he did not think that there was anything hurtful in that. It was the only thing open to the physician. It was only when ordinary remedies had failed that the thing began to dawn upon him. When the physician was first called upon he did not at once see that it was acute obstruction. In all cases of constipation they should avoid severe purgatives, although it seemed to him that the ordinary purgatives were of no hurtful effect. Opium not only quelled the pain but prevented collapse, and also appeared to act as a stimulus to the bowel if it was given in small quantity. Certainly some cases seemed to be relieved by a dose of opium. A great many cases were on record where the steady administration of opium relieved acute obstruction. Enemata were also valuable, although they, too, might irritate as well as purgatives, because they might bring on vomiting, but they were within the physician's reach. He had used massage, and had succeeded in relieving very serious obstruction. The question was when these remedies ought to stop. When does the case assume serious aspects and require other assistance? Now this was a very difficult matter, and each case should be judged upon its own merits. Certain it was that individuals differed as to their tolerance of obstruction. Some seemed to be killed very soon, others lived on for many days. It was, therefore, not easy to lay down rules to each case. He was not very sure whether he was right, but he would suggest that physicians might look upon this matter as surgeons did upon hernia. When vomiting occurred, and when there was no movement of the bowels, it would be time to seriously consider whether something more active should not be resorted to. Certainly when any threatened collapse in the appearance of the patient came, and when the

strength yielded, and if possible before the temperature rose and the pulse got quick, then was the time for the interference of the surgeon. The great rule he thought applicable as regards the physician was that he should put his whole mind and conscience into the case. He should inquire into every point of the history that could throw any light upon it, and should attentively watch the patient. If he had even exhausted that he had certainly done a great deal in arriving at an approximate diagnosis. He should put it to himself thus—"If these symptoms were occurring in me with the knowledge I have of my condition, how should I desire to be treated?" Would he, identifying himself with the case, desire to continue temporizing or palliative measures, or would he desire the interference of the surgeon? He should certainly say that if he felt he had an obstruction which probably depended on a band or volvulus, he would not hesitate in his choice, for in one case die he might, but in the other die he must.

Dr Cathcart said he thought it would help them to come to some practical conclusion if they were to discuss some of the individual symptoms of intestinal obstruction, and try to find out how far some particular kinds of these symptoms might be peculiar to certain forms of obstruction. He would try to confine himself to one of the special symptoms—pain. If they asked themselves what was the cause of abdominal pain in the various conditions, which might be either obstruction or something simulating obstruction, it might be divided into three. In the first place, it might be due to direct injury to some of the abdominal viscera. And that injury might be a mechanical pressure upon the viscera, such as, for instance, the constriction of a hernia, or the pressure of a band, or the pressure of a tumour, or it might be due to some injury of the splanchnic nerves, as in the passage of the gall-stones or renal calculus. In all these there would be acute pain, and in that way they could understand there were a great number of different conditions. But there might be other symptoms which would depend upon the nerve irritation. For instance, they might have vomiting and collapse, or they might have the tense condition of the abdominal muscles. Then they also had pain as the result of inflammation. That inflammation might be primary or it might be secondary,—that was to say, it might be an inflammation which started round some mechanical injury, as in the inflammation which always followed a hernia or an obstruction; or it might be an inflammation which was a primary condition from the first, as in localized peritonitis, or the inflammation caused by irritant poisoning, which was not always very easy to treat. Then they had another cause of pain, viz., muscular spasms, and these might be present in conditions which were actually intestinal obstruction or something similar. For instance, they had a muscular spasm which was the result—which was a symptom

of obstruction. They had a muscular spasm in ordinary colic, where some part of the alimentary canal was distended by wind. Having stated these, they might discuss the various seats of pain. In the first place, pain might be at the points where the lesion occurred, and in a certain number of cases that was so. He was not prepared to say that pain was always localized, but it seemed to him to be more easily localized in the great than in the small intestine. If pain were in the flanks, they would generally find the mischief there; but if it was across the abdomen, the real seat of a pain might be anywhere in the abdomen. It seemed to him to be a very important matter whether the pain was a constant or an intermittent one. From his own observations he would be inclined to say, that pain was more frequently intermittent in the great intestine, and that it was more frequently constant in the small intestine. Treves said that when the obstruction was constant the pain was always constant. He would simply instance, with regard to that, Mr Miller's last case of volvulus. The pain was very acute at first. According to the woman, she walked home doubled up and had extreme pain. He was not told so, but he rather thought she had spasms. In her case intestinal obstruction seemed to be complete, and very tight strangulated hernia would cause great pain by a mechanical injury, and therefore a constant pain was only to be expected, and paroxysmal pain must be left out of account in that case. He had just read a physician's account of a severe strangulated hernia, and he complained of a very constant pain. He had always found that in intussusception paroxysmal pain was a very marked symptom. In a very acute intestinal obstruction the pain was generally constant. Then, as regarded the onset of the pain and the progress of the case, where the pain came on very suddenly, it generally meant that there had been a sudden obstruction, but it did not necessarily mean that there had not been previously an obstruction—it only meant, probably, a sudden development to complete obstruction. Then, so far as he could believe, pain might be very acute at first; but it might be that, as the patient got worse, the pain got less. Then came the question of tenderness. There were cases in which tenderness might be very marked and where pain might not be excessive. There were other cases in which pain might be excessive where tenderness might be comparatively slight. Tenderness seemed to point to more or less peritonitis, and it was the presence of tenderness as a marked symptom, and hardness of the muscles over the affected region, which enabled them to tell between perforation and obstruction. In a case which he saw recently, where there was a rupture, tenderness as well as pain was a symptom. Vomiting was another symptom. It might be due to two things. It might be reflex, due to severe affection of the splanchnic nerves or to some obstruction of the alimentary canal. In one of Mr Miller's cases vomiting occurred very early;

in the other cases there was very little vomiting. It was an obstruction of a band along the great intestine. His belief was that where they got a sudden injury to any part of the splanchnic system they got reflex vomiting, but that where they got vomiting due to intestinal obstruction it would be markedly in the small intestine, and would be a comparatively small symptom of comparatively less importance, as Dr Affleck said, than when it affected the great intestine. Then, as to the character of the vomiting, there was one symptom which, he thought, had been mentioned by Professor Spence,—wherever the patient had got a comparatively mild form of vomiting, merely belching up fluids, that was an extremely bad symptom. He had never seen a case recover where there was belching of fluids without any retching whatever. He remembered a case in which a patient seemed to be going on very well after operation for strangulated hernia, and the first thing observed was that he began to belch up the fluids he had been drinking, and that went on till he gradually sank and died from gangrene of the intestine. What it was due to he could not say, but it was a different thing to forcible retching; there was no strain, apparently, to the abdominal wall. There were one or two of Dr Affleck's remarks he would allude to. In referring to the age of the patient as throwing a light upon the possible cause of obstruction, he did not mention that in children constipation sometimes caused symptoms very like to intussusception. He remembered seeing a case where the physician in charge was just on the point of sending for a surgeon to open the abdomen, but he continued with opiate applications, externally and internally, and repeated enemata, and at last got fæcal evacuations, upon which the whole case depended. It was proved in that particular case that the obstruction was due to a young child being fed upon starchy foods, and he should always make careful inquiry on that point before coming to a conclusion. He referred to the case mentioned by Mr Miller where they gave the patient enemata just before the operation. These enemata passed through the volvulus and went into the cæcum, and yet did not return, because, at the operation, they found a quantity of the fluid, which had been sent into the cæcum, but which had not been able to pass down. Then as regarded the very important question of when to interfere. It all turned upon the question of diagnosis, and their effort should be to try to know what cases should be interfered with and what should not. There were cases, *i.e.*, such as those of obstruction by a band which ought to be interfered with the very moment they presented themselves. There were other cases which should never be interfered with at all—for instance, irritations of the alimentary canal, possibly local peritonitis. The point came to be this,—considering their inability to come to a definite conclusion, what should be the line of treatment to be adopted in all cases where there was a doubt, and at what period

should interference be made? He fancied every one would be of opinion that the line of treatment should be to soothe the parts, to give a diet which should leave almost no solid matter, *i.e.*, to give no milk, to give only arrowroot, and broth, and beef-tea, and to avoid all irritants to the rectum. If it was merely obstruction, the inflammation would be lessened, and if it was merely inflammation, the bowels would move, as soon as inflammation subsided.

Dr W. Russell said he thought, in the first place, that the physician should be able clearly to distinguish between all conditions of enteritis and peritonitis, and to separate them clearly and distinctly from cases of mechanical obstruction in the alimentary canal—that was from cases of obstruction not due simply to paralysis of the bowel. One of the difficulties they had was in deciding which cases would go on to grave obstruction. These cases were exceedingly common, and he had met with many cases in which there had been a continuance of constipation, local symptoms, and a degree of sickness, and in which a bold administration of purgatives had most certainly relieved the symptoms, with the result that the patient was cured. Now, a number of these cases no doubt did turn out to be cases of grave intestinal obstruction; but, as Dr Affleck had indicated, the difficulties of diagnosis here were considerable, and was often determined by the result of treatment. If they did not succeed in removing the obstruction by ordinary treatment, they regarded the case as one of grave intestinal obstruction. As to the causes of grave abdominal obstruction, one had had very varied experience. One of his first cases of this kind was in an old and very fat woman. The cause of the obstruction was quite obscure, and it was discussed whether the abdomen should be opened. The patient was considered a particularly bad subject for such an operation, and surgeons were not so ready then as now to open the abdomen, so she was left to die. At the post-mortem a very large gall-stone was found, which could easily have been removed. Another case of a foreign body was in a child who was suddenly seized with symptoms of abdominal obstruction. The interest of the case lay in the fact that at one part the coils of the intestines were united by old adhesions, and there was in this case an old history of inflammation. At the seat of the adhesions he found that the intestine was occluded by an onion, which the child had evidently swallowed whole. This case also might have been saved by interference. He at once put aside cases of internal hernia and cases of twist. In these cases one's experience would lead one to believe that the symptoms came on so very acutely, that there was an indication in the very acuteness of the condition to lead one to diagnose that probably there was mechanical obstruction to the canal. His experience of chronic obstruction, both in practice and in the post-mortem room, was that these cases were mainly due to malignant stricture of the

intestines; but he had been struck with the fact that in a very large number of cases the stricture had been of such a limited size that it seemed to him to be suitable for removal and for resection of the bowel. This, he thought, was a fact which was worth noting. His impression for some time had distinctly been that there were a great many more cases of this kind than was generally believed. Then there was one other condition which sometimes led to more or less acute obstruction, but which sometimes ranked itself more properly in the cases of chronic obstruction. He referred to cases which were localized in the neighbourhood of the cæcum. Some of these cases, beginning, as he should expect they most frequently did, in the appendix, might be insidious in their onset—that was to say, in their sub-acute stage—going on rapidly to the more acute stage. Now, even in these cases where the lesion could be localized to the cæcum, there was a practical difficulty in knowing when the operation should take place, because a certain number of the cases got better under ordinary medicinal measures. Others, again, presented a history of pain recurring from time to time in the region of the cæcum, and the case might only come under observation in the stage of acute obstruction. Now, he took it that all cases of this kind ought to be recognised by the physician, perhaps more from the history of the case than from the present condition of the patient; and that was a condition which he thought the surgeon agreed with the physician in saying ought to be operated upon without delay. He thought that, after what they had heard from Mr Duncan that night, many of them would be able in the future the better to grasp and better able to classify the cases which they had seen in the past, and which they were certain to see in the future. He also thought that the discussion, although the field was an exceedingly large one, might ultimately tend to a very good purpose.

Dr James Young said that a case was brought to his mind by the remarks of Dr Affleck. The patient presented the symptoms of severe constipation, and was suffering pain. She was about 60 years of age, and had had no action of the bowels for twelve or fourteen days. He thought he might find some uterine affection, but that was not the case. On palpation of the abdomen there was distension, but no localized pain. On examination of the rectum he found, on passing the index finger, a stricture or tight contraction of a circular character. As the case was urgent, and the patient in great distress, he divided the band very cautiously, and a fæcal discharge followed. That same afternoon he passed a rectal bougie, and the patient has been well ever since. There has been no return.

Dr Wallace wished first to make a remark, and secondly to ask a question. The symptoms in obstruction varied according to

whether the large or small intestine was the seat of the obstruction, and he wondered whether some light might not be got as to the seat of obstruction by contrasting the symptoms of strangulated hernia with those of intestinal obstruction. In hernia the small intestine was commonly the part affected, and was it not possible that the symptoms of complete obstruction in the small intestine would closely resemble those of hernia, in which there was complete strangulation, while partial obstruction in the small gut would be analogous to the Littre variety of hernia—the symptoms in both varieties differing from obstruction of the large gut. Recently it had been advocated, in cases where the patient was suffering from obstruction, but in a condition of great collapse, that *enterotomy* should be performed. A small incision to be made in the inguinal region, and the first piece of dilated intestine felt stitched to the abdominal wound. By that means the patient was saved as much as possible from shock by the operation. He would ask Mr Miller if that would not be a good operation to perform?

Dr Charles Kennedy said that last week he had a case of intestinal obstruction. The first symptoms were vomiting and constipation, obstinate. Strong purgatives had been given without effect. When he saw the child an enema gave a fair motion, and apparently gave the child complete relief for over twelve hours. After twelve hours, however, severe pain came on again, the child screaming, and symptoms of obstruction recurred. The most important point was that tenderness was present, indicating local peritonitis over the right flank. The vomiting was only present for a short time. Pain apparently was intermittent. He was unable to make up his mind as to the exact nature of the case. The child suddenly died, and on making the post-mortem he found that the cause of obstruction was an adhesion to the small intestine, quite near to the cæcum, and that the lower part of the ileum had slid through. The peritonitis was quite local, and was caused apparently by a caseous gland, of which there were a good many in the abdomen, to which the omentum had become adherent. What misled him largely in the diagnosis was the feeling of resistance in the right flank, the tenderness there, and the dulness on percussion in that position. Of course it was in his mind at the time to have operative interference, but the sudden death of the child prevented that.

Mr F. M. Caird recollected being called to operate in a case supposed to be volvulus. He could not come to the conclusion that it was so, but opened the abdomen, and met with a great mass of tubercular glands, and it was easy then to make the diagnosis of commencing tubercular meningitis. In another case the diagnosis arrived at was probable intussusception, but after waiting, the child was relieved of a large mass of impacted fæces under

chloroform. As an example of obstruction due to malignant disease, he mentioned a case in which he cut out a section of gut, including an annular stricture of the descending colon, with deposits at a higher level. In two cases associated with inflammation of the vermiform appendix, the pain was referred to the region of the umbilicus, and it was only as the cases advanced that one was able to notice the fixation of the abdominal parietes in the iliac region during respiration. What struck one in many of these cases was that occasionally temperature was not very high, and that although the patient perhaps experienced great relief after a copious and extremely offensive stool, immediately afterwards there was not a relief, but a development of at least peritonitis; and the patient sank, and then it was found that there had been an abscess. One would be tempted under those circumstances, in the event of such a history, and in the event of being able to localize, where it was not taken at the beginning, to make an incision in the region of the appendix, and to try to enter the cavity, and to wash it out and introduce a drainage-tube prior to the rupture, and then to go on and open in the middle line and wash it out. He was sorry he could not say that he had met with any success whatever in the performance of abdominal section for the relief of intestinal obstruction.

Dr Harold Stiles put a question regarding the pathology of intussusception. He wished to know, in those cases of intussusception beginning at the ileo-cæcal valve, what part of the gut was felt *per rectum*, and if the ileo-cæcal valve could be felt in that position?

Dr Scott Lang said Mr Treves's view in cases of intussusception at the ileo-cæcal valve was that the intussusception took place at the expense of the entering tube, and the position of the large intestine remained constant. There were three tubes—the receiving, the entering, and the reflected tube. When intussusception took place at the expense of the entering tube, and the ascending colon remained in its place, the ileum was practically doubled down upon itself; but in all other cases the intussusception went on at the expense of the reflected and receiving tubes. He was not sure, however, that this had been verified by post-mortem.

Dr Duncan mentioned a case in a child on whom he performed laparotomy, and reduced the intussusception by compressing upwards. He could feel it through the rectum, and when he opened the abdomen in one case he found a very remarkable state of affairs. The small intestine was invaginated, but the cæcum and the small intestine had passed into a second intussusception. There was a double intussusception, first the small intestine right round, and then a second.

Mr Miller, in concluding the discussion, said that since he had opened the debate, he had looked up Treves's article in the *Dictionary of Surgery*, and he found everything that he could imagine laid down in the most succinct and complete manner as regards the surgical aspect. Everything, he thought, had been written that could be written upon this subject, and he was very much pleased that the idea of giving prominence to the diagnosis had proved of value in bringing forth so many valuable remarks.

Meeting XIII.—July 3, 1889.

Dr John Smith, *President, in the Chair.*

I. Election of New Members.

The following gentlemen were elected Ordinary Members of the Society:—Hugh Jamieson, M.B., C.M., Edinburgh, and David Gair Braidwood, M.B., C.M., Bonnyrigg.

II. Exhibition of Patient.

Mr Francis M. Caird showed a case of transplantation of bone after compound fracture and trephining. T. L., æt. 27, private Cameron Highlanders, was admitted to the Royal Infirmary early on the morning of 24th March 1889, suffering from a compound depressed fracture on the *right side* of the head, rather in front of the upper part of the coronal suture. The patient was unable to speak, but he looked quite intelligent, and carried out various actions, such as putting out his tongue, raising his hand, etc., when asked to do so. Under chloroform, it was found necessary to remove two circles of bone and an intervening bridge before the dura rose to its normal level, and before sufficient access could be obtained to get rid of detached fragments. The trephine discs were cut up after Macewen's method and replaced on the dura; the flaps were laid over them and sutured. A further examination now showed a cause for the aphasic condition of the patient in the shape of a small penetrating wound on the *left side* of the head, $3\frac{1}{2}$ inches above and $\frac{1}{2}$ inch in front of the external auditory meatus, and this led down to a punctured fracture. An incision permitted the elevation and removal of the depressed fragments, and an aperture in the skull large enough to admit the finger tip was left. The dura had been wounded. As some hair and grit had been driven into the wound, it was not thought safe to attempt transplantation of the bone. Towards morning the patient became conscious, and could speak. He gave an account of the assault by which the fractures were caused, and expressed surprise that he had been unable to speak,

but stated that he felt assured that he could have written what he desired to say. Recovery was uninterrupted, the wounds remaining aseptic. At present there is no formation of bone at the left wound, and the cicatrix pulsates distinctly. On the right side, however, the gap is largely filled by bone, and there is only slight pulsation.

III. Exhibition of Pathological Specimens.

1. *Dr Maxwell Ross* showed a specimen of EPITHELIOMA OF THE LARYNX which had been removed from the body of a Goorkha soldier at Mandalay by Dr W. B. Bannerman, of the Indian Medical Service. The whole left side of the larynx from the ary-epiglottic fold to a point half an inch above the vocal cord was involved in the disease, as also the arytenoids of both sides. On the outer aspect of the box of the larynx a mass of cancerous infiltration had formed. Below this, and partly in its substance, was an abscess cavity which extended between the cricoid cartilage and lower border of the wing of the thyroid. Beneath the left arytenoid was an ulcerated spot communicating with the abscess, through which it had burst into the larynx, causing death by suffocation. The left inferior corner of the thyroid cartilage was eaten away, and dislocation of the crico-thyroid articulation produced. The thyroid gland was greatly enlarged. The case was interesting from the complication of the malignant disease with abscess and bronchocele. A full account of the clinical symptoms and post-mortem appearances was published in the *Brit. Med. Journal* for 22nd September 1888.

2. *Dr Harvey Littlejohn* showed—(*a.*) A RUPTURE OF THE HEART through the wall of the left ventricle. The specimen was obtained from the body of a man, æt. 64, a patient of Dr T. A. G. Balfour. Up to a week before death he had not shown signs of heart affection. He then felt pain in the præcordial region, for which Dr Balfour ordered rest in bed. The day before death a peculiar whirring murmur was heard a little below the apex beat. On the following morning, when raising himself in bed to have his breakfast, he suddenly expired. The rupture of the left ventricle was found post-mortem. (*b.*) AN ANEURISM OF THE AORTA from the body of a man who fell down dead in the street. Blood was seen to come from his mouth, and post-mortem his stomach was found to contain about a gallon of blood. There was also a rupture of the mucous membrane at the cardiac end of the stomach about two inches in length, evidently caused by the pressure of this large quantity of blood. The aneurism had eroded the vertebræ from the eighth to the eleventh, and had ruptured into the œsophagus.

IV. Original Communication.

THE MINUTE ANATOMY OF THE PLACENTA IN EXTRA-UTERINE GESTATION.

By D. Berry Hart, M.D., F.R.C.P. Ed., F.R.S.E., Lecturer on Midwifery and Diseases of Women, Surgeons' Hall, Edinburgh; Assistant Obstetric Physician, Royal Maternity and Simpson Memorial Hospital, Edinburgh; Assistant Gynæcological Physician, Royal Infirmary, Edinburgh.

I have first to define the limits of the present paper. I do not take up the anatomy of the extra-uterine placenta in the various varieties of this condition, but consider merely its development in what I have termed the extra-peritoneal form. I must, therefore, briefly explain what is meant by this.

As all know, the Fallopian tube is, in the vast majority of cases, the starting point of an extra-uterine gestation. The most common result of this is that rupture occurs, usually at the second month, through some part of the tube covered by peritoneum, a result almost invariably fatal if left alone, and as invariably curable if operated on in time by abdominal section.

A rarer termination in Fallopian tube gestation is, that further development takes place between the layers of the broad ligament, which become separated to accommodate fœtus and placenta. Even here rupture into the peritoneum may occur; but if the peritoneum remain intact we may get a continuance of development as follows:—

If in the early Fallopian tube gestation the fœtus lies above the placenta, we may get both it and the placenta developing between the layers of the broad ligament. The placenta in its growth has its downward displacement soon limited by the pelvic muscles, while the fœtus may rupture through the peritoneal lamina and lie in the peritoneal cavity free among the intestines. This gives us, therefore, a mixed form of gestation, the fœtus being intra-peritoneal, the placenta extra-peritoneal. The placenta thus develops in the broad ligament and pelvic connective tissue, and has in addition been displaced downwards by the fœtus, the amount of displacement being necessarily small. If, however, in an early tubal gestation the fœtus lies lower in the tube, the placenta thus occupying the part of the tube covered by peritoneum, we may then get a development between the layers of the broad ligament as in the previous instance, but with this special difference. The growing fœtus pushes up the upper part of the broad ligament with placenta, and thus displaces the placenta up until it may lie attached to anterior abdominal wall opposite the level of the lumbar vertebræ. The fœtus and placenta too, lie, therefore, in extra-peritoneal tissue, the peritoneum being lifted up as the pregnancy goes on. This extra-peritoneal development

of extra-uterine gestation is remarkable, but is not unique in pathology, being analogous to the development of a burrowing pelvic abscess or of a papillomatous ovarian cyst. (Fig. I.)

We may sum up the stages, therefore, as, (*a*) Fallopian tube; (*b*) broad ligament in the first place; then (*c*) either placenta extra-peritoneal, fœtus intra-peritoneal; or (*d*) both extra-peritoneal.

If we now take up specially what is more properly the subject of to-night's communication, the minute anatomy of the extra-uterine placenta in its extra-peritoneal development, we must keep in mind the following points. While the placenta begins its development in the mucous membrane of the Fallopian tube, it soon develops in connective tissue; and during its development here undergoes a slow displacement. In the case where both fœtus and placenta are extra-peritoneal, the fœtus lying below, the placenta must have been displaced upwards for at least 10 inches. Inasmuch as this amount of displacement was accomplished in about as many months there was never any gross separation, but a slow microscopic progress which, as we shall see, causes slow blood effusion and organization.

In investigating the minute structure of the extra-uterine placenta I had the following material:—

1. Tube with placenta attached from a ruptured tubal gestation at the second month.
2. Broad ligament gestation at fourth month.
3. Abdominal gestation, entirely extra-peritoneal, and at nearly full time.
4. Abdominal gestation, with fœtus probably intra-peritoneal, and placenta certainly extra-peritoneal.

The last preparation I owe to the great kindness of Dr Barbour, and no one appreciates more than I do the self-denial which gave me such a valuable specimen.

The tubal gestation was cut in paraffin, the others in celloidin, and the investigation was carried on in the laboratory of the Royal College of Physicians.

The following is a brief summary of the results:—

In the Fallopian tube gestation the villi lie embedded in decidual cells, and no intervillous sinus system seems to exist. Large sinuses, however, have formed in the muscular wall. The villi are well formed, and are covered with perfect epithelium. The decidual cells are large, and have large nucleus and nucleoli. (Figs. II., III., and IV.)

In the broad ligament placenta we begin to see the serious damage caused to placental structure by its development in connective tissue. The villi are less perfect in contour, blood extravasation is present, blood crystals are in abundance, while the decidual cells are few and less perfect.

In the advanced abdominal gestation where the placenta is in the pelvic connective tissue, and therefore less displaced, we see fairly perfect villi, decidual cells, blood crystals with large areas of

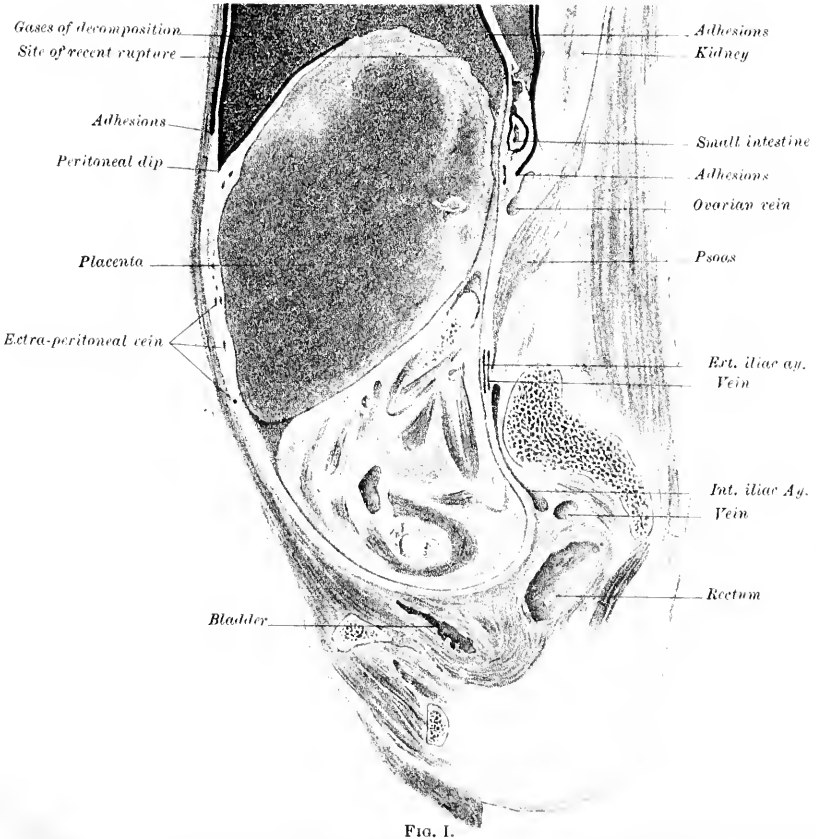


FIG. I.

Sagittal lateral section of advanced extra-peritoneal extra-uterine gestation (Hart and Carter).

FIG. II.

Villi and serotina of two months Fallopian tube gestation (× 75).

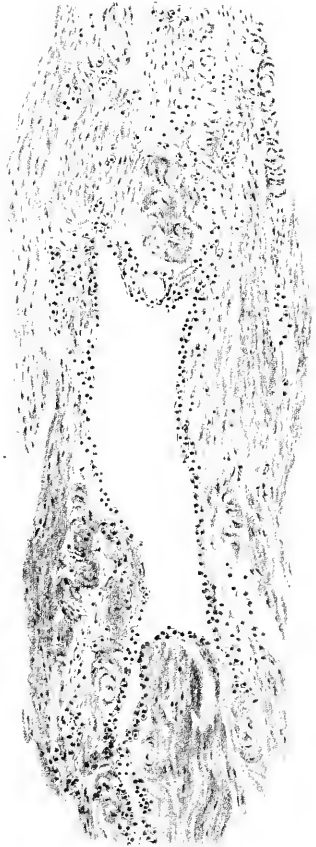


FIG. III

Enlarged vein in muscular wall of Fallopian tube gestation (× 75).

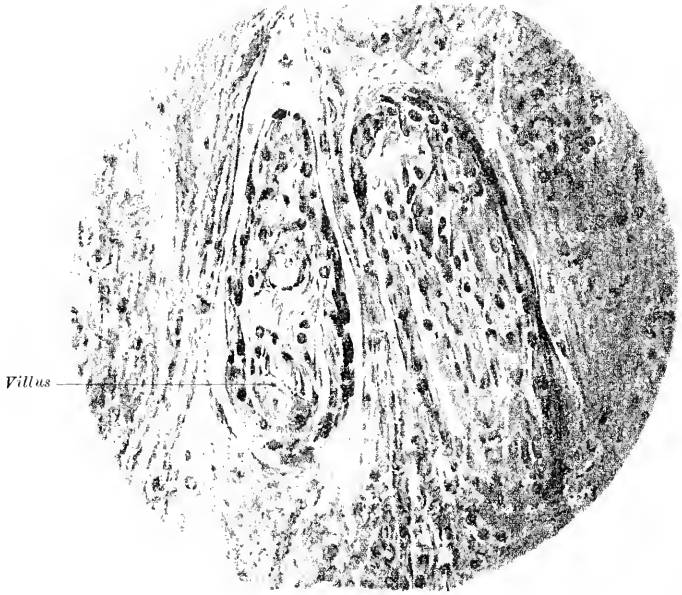


FIG. IV.

Villi and serotina of two months Fallopian tube gestation (× 300).

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FIG. V.

Placenta in advanced extra-peritoneal extra-uterine gestation (× 25).

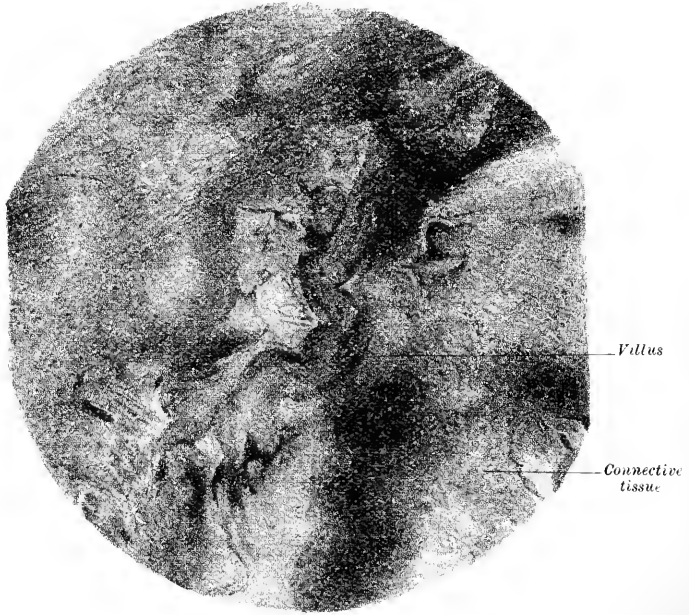

FIG. VI.

Villus in organized blood clot of advanced extra-uterine gestation (extra-peritoneal) showing compression and distortion (× 300).

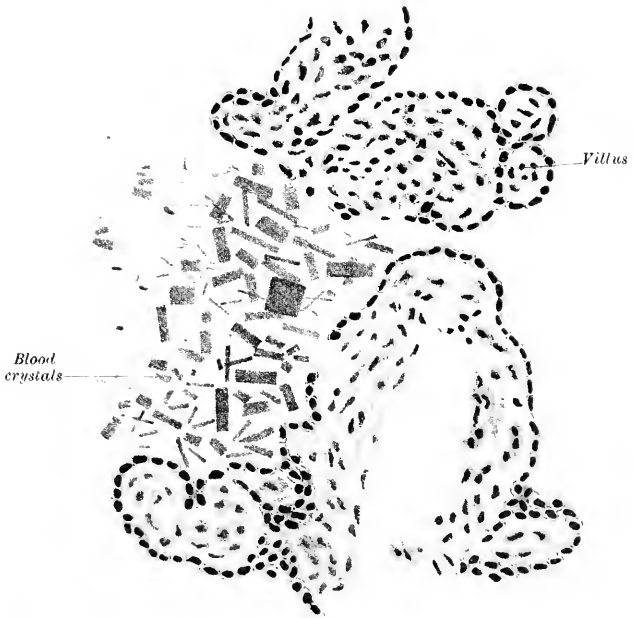

FIG. VII.

Villi and blood crystals of advanced extra-peritoneal extra-uterine gestation (× 280).

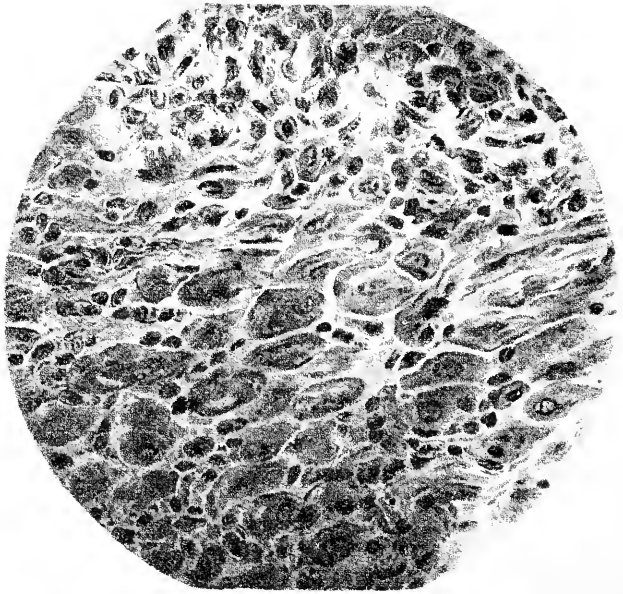

FIG. VIII.

Decidual cells from uterine decidua of broad ligament gestation (4½ months) (× 375).

extravasated blood, which may be the remains of an attempt at a placental sinus system.

The most remarkable alteration in placental structure is found, however, in the abdominal gestation with the extreme displacement of the placenta. Here the placenta is converted into a mass of organizing blood clot, with large areas of blood crystals, great compression and distortion of the villi, entire absence of decidual cells, and no recognisable intervillous sinus system; although the enlargement of the veins in the abdominal wall at the placental site points to some such arrangement existing. (Figs. V., VI., and VII.)

We may, therefore, say broadly that the development of the placenta in the extra-peritoneal form of extra-uterine gestation is a destructive one, reduces the placenta practically to compressed villi, where the serotina is destroyed and replaced by blood crystals and organizing blood clot. The least damage is done to the placenta, which from its more favourable position in the lower part of the tube with the fœtus above, is, as I have already explained, displaced least.

In the last place, I have to point out the important bearing of these facts on the life of the fœtus. One of the most common terminations of an extra-uterine gestation is to have the fœtal bones discharged per rectum, or in fact to terminate as a pelvic abscess. Such are evidently extra-peritoneal developments of a Fallopian tube gestation, where the destructive process going on in the placenta has killed the fœtus, and where the proximity of the whole gestation to bowel unprotected by peritoneum has led to an endosmosis of intestinal gases, or passage of micro-organisms such as to determine putrid suppuration.

A more favourable termination is where, from the higher position of the fœtus, we get its escape into the peritoneal cavity, less displacement of the placenta, and thus less interference with its functions. These are the cases where a full time fœtus can be removed, and where the placenta, from its extra-peritoneal position, is not recognised at the time and is left undisturbed for ultimate absorption if away from bowel. Proximity to bowel may lead to its putrefaction and breaking down.

Description of Figures.

Fig. I.—Sagittal lateral section of advanced extra-peritoneal extra-uterine gestation (Hart and Carter).

Fig. II.—Villi and serotina of two months' Fallopian tube gestation.

Fig. III.—Enlarged veins in muscular wall of Fallopian tube gestation.

Fig. IV.—Villi and serotina of two months' Fallopian tube gestation.

Fig. V.—Placenta in advanced extra-peritoneal extra-uterine gestation.

Fig. VI.—Villus in organized blood-clot of advanced extra-uterine gestation (extra-peritoneal).

Fig. VII.—Villi and blood crystals of advanced extra-peritoneal extra-uterine gestation.

Fig. VIII.—Decidual cells from uterine decidua of broad ligament gestation (4½ months).

Dr Barbour considered that the important point Dr Berry Hart had brought forward in his paper, and been the first to demonstrate, was that gestation might go on to an advanced period when the fœtus was extra-peritoneal. He was not quite clear that in Fallopian-tube gestation the fœtus should be considered a displacing cause, as advocated by Dr Hart. It lay in amniotic fluid, and was unable to cause displacement in the way suggested. It was a very remarkable thing to have cellular tissue, as in the broad ligament, taking on the functions of the uterine mucous membrane, and supplying a maternal placenta for the extra-uterine fœtus. He was not certain that there was proof to show that pregnancies in which the placenta was found attached to abdominal wall or intestines were Fallopian-tube gestations to commence with.

Dr Haycraft thought the more one could learn about the placenta in these abnormal conditions the better would its functions in the uterus be understood. It seemed to him that the placenta in its new position performed exactly the same functions as in utero. The interest of this was as bearing on the question of uterine glands. In the Fallopian tubes they had a structure very similar to the uterine mucous membrane, but no glands. The glands in the uterus were supposed to secrete fluids, and he did not think every one was perfectly satisfied that they played no part in gestation. It seemed a matter of particular interest to find that in these cases, practically without the assistance of uterine glands, in simple connective tissue the fœtal villi were able to produce a placenta without the same structures one found in the uterus.

Dr Berry Hart, answering Dr Barbour's criticism as to the fœtus being a displacing cause, held that in extra-peritoneal extra-uterine gestation there was little or no liquor amnii. He could see no other explanation for cases of extra-uterine pregnancy with extra-peritoneal abdominal wall attachment of the placenta than that it had begun in the Fallopian tube, passed between the layers of the broad ligament, and developed extra-peritoneally. He had not fully considered the question of the functions of the placenta in connexion with these investigations, but the Fallopian tube structure was histologically similar to that of the uterine mucous membrane. Perhaps the use of the term "uterine glands" was a begging of the question, as they were little more than pits or depressions in the mucous membrane, and similar pits were found in the Fallopian tube.

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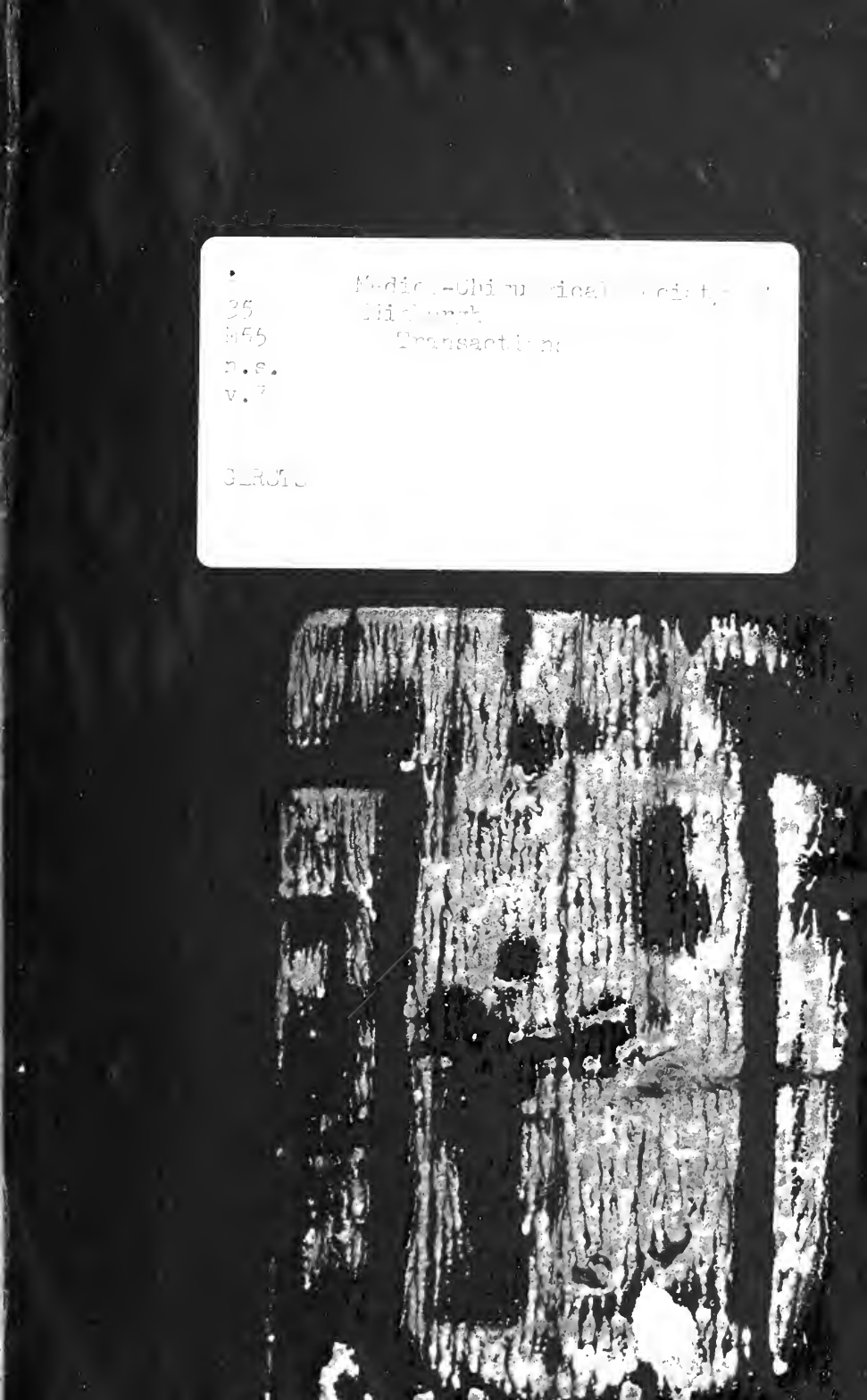
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